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THE

DUBLIN JOURNAL

OF

MEDICAL SCIENCE;

EXHIBITING

A COMPREHENSIVE VIEW

OF THE

LATEST DISCOVERIES

IN

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The Reader is particularly requested to correct the two following ERRATA in the last Number.

In Mr. Moore's Statistical View of the Principal Medicines prescribed in Dublin, at p. 26, *for* nitrate of soda, nitrate of potash, and nitrate of ammonia, *read* citrate of soda, citrate of potash, and citrate of ammonia.

At p. 93, the paper on Amputation of the Penis is said to be by J. THOMPSON, whereas it was written by J. MORRISON, M. D., Surgeon to the Newry Dispensary and Fever Hospital.

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ADDENDA ET CORRIGENDA.

The Editors of the Journal wish to call the attention of their surgical readers to one *erratum* in the translation of Dieffenbach's paper, in the last number (29,) throughout instead of *insect-needles*, read *insect-pins*.

It is important to notice this, for in making certain sutures, Dieffenbach employed the very slender pins used for transfixing insects.

From Dr. SIMPSON not having had an opportunity of correcting the press, the following *errata* have been committed in the printing of his paper on the Spontaneous Amputation of the Limbs of the Fœtus in the same Number.

Passim, *for* Lagorsky, *read* Zagorsky.
Page 225 line 9, *for* Mangelhaste *read* Mangelhafte.
—— 225 — 14, *for* from the bone, *read* from the leg.
—— 228 — 9, *for* there is in, *read* therein.
—— 230 — 25, *omit* happen to.
—— 236 — 21, *for* capable of, *read* not only capable of.
—— 239 — 14, *for* subjacent, *read* subsequent.

THE

DUBLIN JOURNAL

OF

MEDICAL SCIENCE,

1 SEPTEMBER, 1836.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Cases of Anomalous Affections of the Larynx requiring the Operation of Tracheotomy.* By WILLIAM HENRY PORTER, Surgeon to the Meath Hospital, Lecturer on Anatomy and Surgery in the School of Anatomy, Medicine, and Surgery, Park-street Dublin.

[Communicated to the Surgical Society of Ireland April 16, 1836.]

IN investigating the pathology of diseases of the larynx and trachea, I have more than once had occasion to observe that the functions of respiration may be so interfered with as to endanger existence, and to render the operation of tracheotomy absolutely imperative, without the surgeon being able even to offer a conjecture as to the probable cause of the obstruction. Some of these cases are obviously examples of mere lesion of function, inasmuch as the patients subsequently recover the perfect use of the larynx both as to respiration and speech: whilst with others the lesion is probably organic, for they are obliged, through the remainder of their lives, to exist by means

of artificial respiration. As yet I can throw no light on the pathological causes that occasion the phenomena alluded to, because I have had no opportunity of *post mortem* examination; yet were the cases particularly interesting to me, as exhibiting the diseases of this important organ in a new and hitherto unobserved form, and affording abundant material for speculation. I believe that some analogous, if not identical, affections have been observed in the inferior animals. I have been informed that the Wapiti deer that was tracheotomized last summer by Mr. Crampton, exhibited no disease of the larynx after death; or at least not sufficient to explain the urgency of the symptoms which rendered the operation necessary, and which were obvious to every spectator. The disease of the horse, termed "a roarer," also bears some resemblance to these affections, except that I am not aware of its ever proceeding to the extent of bringing the animal's life into peril.

The first case of this description that attracted my attention is already familiarly known, having been published by Dr. Evanson, in the Dublin Journal of Medical and Chemical Science, March, 1834. It referred to a child aged one year and one month, who was operated on by Mr. Crampton, in the Meath Hospital, under circumstances of peculiar urgency, who experienced great relief, and eventually recovered; having left the hospital without any cause that could be relied on being discovered to explain the excessive difficulty of breathing. I refer to this case with some diffidence, because the infant's mother stated afterwards to Dr. Evanson, that "she had herself extracted a fish bone from the windpipe," but concealed the circumstance through fear that farther operation might have been resorted to. At the time, however, and to this moment I did not and do not attach credit to the woman's story, and I am happy to find, on perusing the case, that similar doubts, as Dr. Evanson candidly relates, were entertained by Mr. John Hamilton, who attended the little patient with the

utmost assiduity, and had an opportunity of observing every symptom from the time of its admission into Hospital.

Whether there was really a fish-bone in the infant's larynx or not is now a matter of little consequence, for the history of the case is involved in so much obscurity that no reliance can be placed on it. I merely mention it as having first directed my attention to the circumstance of a patient being brought to the verge of dissolution by obstructed respiration, without any cause discoverable either at the time or afterwards. The second case occurred in a patient under my own care in the Meath Hospital: a boy named James M'Mahon, æt. fourteen. Two months previous to admission, whilst in the act of eating some beef hash, he had been suddenly seized with all the symptoms of threatening suffocation, which continued with alternations of remission and exacerbation until the 29th August, 1833, when the operation became necessary, in order to prolong life. No foreign body of any description was extracted or discovered. On the 6th September, (eight days after the operation,) it was found that the measures adopted for subduing inflammation, or restoring the larynx to its healthy functions, had completely failed. The moment a finger was laid on the orifice of the tube, respiration ceased: if the tube was withdrawn for an instant, the convulsive breathing was renewed, and it was evident that the obstruction (whatever it might be) was situated above the wound, and probably in the immediate neighbourhood of the rima glottidis. I resolved on making a close examination of the parts, which I did by means of eye and probe, without making any discovery: but still dissatisfied, I passed a large-sized bougie from the mouth, through the rima, and out by the mouth. The instrument passed with facility from above and from below, and not the slightest indication of the existence of any mechanical obstruction could be observed. Whilst passing the bougie from the wound upwards, the struggles of the patient brought the epiglottis, the glottis, and even the rima distinctly into view, but nothing unnatural or unhealthy could

be seen: yet the larynx remained altogether incapable of performing its function of transmitting air to the lungs.

It is unnecessary to enter into further details relative to this case, as an outline of it also has been published in the Dublin Journal of May, 1834. I may, however, add to what has been already there stated, that the boy was obliged to wear the tube for more than nine months: that whenever he attempted to dispense with its use, or that it slipped from the wound accidentally, he was brought into the greatest danger from obstructed respiration, with frightful exacerbations of an apparently spasmodic character: and that three or four times (once in the middle of the night) he was indebted to professional assistance for the preservation of his life. Yet is this boy now quite well; the wound in his neck healed; his breathing natural, and the only inconvenience he experiences is a peculiar hoarseness of voice, which is (I believe) a necessary sequela to all severe affections of the larynx.

The cause of the obstructed respiration was never satisfactorily discovered, nor was there any reasonable explanation offered on the subject. At the period when it was at its greatest height, the bougie passed with the utmost facility through the rima, which, as well as all the adjacent parts, seemed to be perfectly healthy.

The next case is one of an opposite character, for the subject of it never recovered the power of breathing through the larynx: he respires still through the artificial opening, and to this day exhibits several peculiarities, which I think not undeserving of attention.

Michael Lyons, æt. fifty, came to the Meath Hospital at three o'clock of the morning of Saturday, December 6, 1834, in the following miserable condition. Respiration sonorous and laborious; a peculiar harsh stridulous sound accompanied each act of inspiration; patient most anxious, restless, and uneasy; face flushed, eyes staring as if protruded, lips swollen but pale; larynx and trachea moved rapidly upwards and downwards in

the neck; he could not lie down even for a moment, the slightest attempt to do so inducing an immediate paroxysm of suffocation; his voice was nearly gone, the attempt to articulate amounting only to an indistinct whisper; and he had occasional exacerbations of dyspnœa that greatly aggravated his distress.

He stated that in the evening he had gone to bed quite well, but awoke about twelve o'clock with difficulty of breathing, a feeling of dryness and huskiness in the throat, frequent cough, and a sensation as if some extraneous substance was in the fauces. As the difficulty of breathing increased, he felt a most painful sense of constriction in the larynx, on every attempt to inflate the lungs. On admission, thirty ounces of blood were taken from his arm, and he was ordered a quarter of a grain of tartarized antimony every twenty minutes. By these measures he was so much relieved, that at nine o'clock (the visiting hour) he was found tranquilly asleep. He was then ordered calomel and opium.

On the 7th I find the hospital report to have been, "passed yesterday and last night tranquil; difficulty of breathing almost gone," but from this moment the aspect of the case assumed a decided change, and the difficulty of breathing increased rapidly; notwithstanding that the mercury had made an impression on the system, as evidenced by the fœtor from the mouth, it had made none on the disease: tartarized antimony was rubbed on the neck with as little effect. He passed the day of the 10th most wretchedly; during the night experienced a terrific paroxysm which had well nigh carried him off, and on the 11th I was obliged to perform tracheotomy. I had never performed an operation with less immediate relief than this, principally on account of the difficulty he experienced in expectorating mucus, which had accumulated in enormous quantity. Although this circumstance does not bear upon the point immediately in question, I mention it, because I was near losing two patients before from this cause, which may, I believe,

be avoided by not delaying the operation too long. This patient went through the usual course of symptoms, on which I need not dwell: he had bronchitis twice, from which he recovered; and he suffered great inconvenience from the closing of the wound, which it was necessary to dilate two or three different times. At the end of three weeks no progress whatever had been made towards a restoration of the natural passage for the air. On placing his finger on the tube, he could expel the air through the rima with great facility, and even speak in a rough and raucal tone; but inspiration was impossible; the attempt was accompanied by a loud stridulous sound, and it was evident that little air could pass the rima, or gain admission into the thorax. The phenomena of this attempted respiration were exactly such as might be occasioned by a valve placed above the rima, which would easily yield to a column of air passing upwards from the lungs, but which would be steadily and firmly closed by the pressure of the air in any attempt to inspire.

It should be borne in mind, that in all laryngeal affections, the chief difficulty of breathing is experienced in the act of inspiration. The object of the sufferer is to fill his lungs as completely as possible; to this all his energies are directed, and hence the effort in this part of the function is long and painful, and the stridulous sound most distinct. This is observable even in cases where the change of size and shape in the rima have been produced by ulceration or other organic disease, and where the obstruction being mechanical, should operate as much against the exit as the entrance of the air. Of this fact I was well aware, but I had never before remarked any thing like the difference between inspiration and expiration that this case exhibited. I endeavoured to investigate the cause as accurately as possible, and in this patient also passed a bougie with the greatest freedom from the wound upwards into the mouth. I could not pass it from above, by reason of the violent irritation produced, and the dreadful fits of coughing that supervened whenever the instrument entered the larynx. On one occasion,

during the struggles of the patient, I was enabled to see the glottis, and could not perceive any deviation from healthy structure or shape.

This man remained in hospital for several weeks, and underwent a complete mercurial course without the smallest benefit, during which time I watched with the utmost anxiety for any probable indication of the nature of the mischief within. There was no pain on pressure in the vicinity of the larynx; no difficulty of swallowing; nor any discharge of purulent matter by the mouth or the wound. Finally he left the hospital, still breathing through the tube, and apparently condemned to abide by its use during the remainder of his life.

In the beginning of February, 1836, (fourteen months after the operation,) I was called very early one morning to this patient, who had applied to the hospital in a state threatening suffocation; the wound had become narrow and contracted, and the tube having slipped out could not be re-introduced. The symptoms now shewed how totally incapable he was of carrying on respiration by the glottis, for even with the assistance of the now-diminished aperture in the trachea, inspiration was so difficult as to be performed with an almost convulsive struggle, and with a curious stridulous and hissing noise. I dilated the former wound upwards and downwards, an operation that occasioned such profuse and troublesome hæmorrhage, that I was obliged to have recourse to the actual cautery to restrain it. He remained in hospital for several days, still exhibiting the same phenomena of difficult inspiration, the act of expiration being comparatively easy; and finally was discharged, still breathing through the tube. I have seen him frequently since, and he remains in the same state.

I have often reflected on these cases as furnishing a most instructive lesson, shewing how impossible it may be to determine on the pathological condition (during life) of a larynx so obstructed as to render an operation imperative, and how difficult to prognosticate the result. That of the boy (M'Mahon)

had every appearance of being a case of chronic inflammation, occasioned by the pressure of a spicula of bone, or other foreign body, and from its long continuance likely to have produced ulceration or other organic derangement even before the operation was performed. Yet no foreign body was ever discovered; no cause could be assigned for the commencement, or continuance, or (least of all) for the termination of the disease, inasmuch as he recovered perfectly; an occurrence not of great probability if either abscess or extensive ulceration had existed in the larynx. That of Lyons offered reasonable grounds for supposing it to be a case of acute cynanche laryngea, threatening to terminate in œdema of the glottis. It had made its appearance suddenly while the patient was asleep, and reached a fearful height in a very short space of time. The effect of the antiphlogistic treatment in the commencement seemed still farther to countenance this idea; yet the event proved the existence of some organic and probably permanent derangement of shape or structure in the glottis.

On the 22nd February, 1836, another case was admitted into the Meath Hospital, which was equally obscure as to its pathological character. Robert M'Donnell, æt. 24, of a pale unhealthy appearance, but nevertheless, by his own account, by no means of delicate constitution, had, ten weeks previous to this period, been attacked with fever, which continued during four weeks, and terminated in imperfection of voice and difficult respiration. These symptoms gradually increased in intensity up to the day of his admission, when he was observed to suffer from great dyspnœa, accompanied by a harsh and stridulous sound, spasmodically exacerbated at night, and increased by the slightest exercise. He was obliged to sleep sitting up in bed. He had frequent paroxysms of cough, attended with expectoration of a small quantity of mucus, slightly streaked with blood, and expelled with difficulty.

Pulse very frequent and small; 120 in the minute. This man resolutely denied ever having had a venereal symptom.

He was ordered a scruple of calomel three times a day. On the 25th his mouth was decidedly affected by the medicine, but without any alleviation of the laryngeal symptoms. On the 26th there was profuse ptyalism; and on the evening of this day I received a note from the resident pupil, requiring my attendance immediately. I went, and was obliged to operate.

I had seldom seen an operation more easily performed, (though at night,) or more decidedly successful. In a few minutes he became quite composed and tranquil; he then had some refreshing sleep; and expectorated so freely through the wound, that he subsequently required less care and attendance than is usually requisite in these cases. He complained of pain and difficulty of swallowing, which I was disposed to attribute to the mercury, and he had a slight attack of bronchitis, from which he speedily recovered. The examination of the chest by mediate auscultation indicated very favourable results: for although the laryngeal symptoms led to a suspicion that an abscess had occurred after fever, yet the lungs being healthy I entertained the less apprehension on that account, for I am convinced that phthisis laryngea is more frequently complicated with pulmonary disease than is generally supposed, and that the fatal result of such cases after operation is usually attributable to the latter cause. That phthisis laryngea is not in itself necessarily destructive has been proved by a multitude of cases, amongst the rest in my own experience by that of Brady, (published in the 5th vol. of the Dublin Hospital Reports,) who was operated on in the month of November, 1829, and who is still living and breathing through a tube. Perhaps I may be here permitted to notice a stethoscopic phenomenon, which to me at least appeared curious. In general when a patient is breathing through a tube, the rush of air passing through the instrument masks and confounds every sound within the

chest; nor can the character of the respiration be ascertained except by examining the most inferior part of the lung. In this case, however, the passage of the air through the tube was very free and scarcely occasioned any noise, and on applying the stethoscope to the chest no sound whatever could be heard; there was an absence of respiratory murmur. This was observed by Dr. Stokes on the seventeenth day after the operation.

Impressed with the idea that an abscess existed somewhere in the vicinity of the larynx, I watched most anxiously for any demonstration of the correctness or fallacy of my opinion. I sought for any evidence of the expectoration being purulent by making him at one time discharge the sputa into a basin of cold water, and at another into a vessel containing a solution of the muriate of ammonia. I examined its smell in the expectation of meeting with the peculiar fœtor that attends the discharge in phthisis laryngea. I thought once that I had detected one of the arytenoid cartilages, but it proved to be only a portion of inspissated mucus; and the sputa was occasionally streaked with blood, which however, came from the back of the trachea, rubbed and irritated by the tube, which happened to be a little too long. I even passed a bougie from the wound upwards into the larynx, in the hope of rupturing the abscess, a manœuvre in which I had succeeded once before, and I carefully inquired every day, whether any portion of the patient's drink had passed out through the wound. All was in vain; and this again seemed to be one of those anomalous cases in which symptoms of obstructed respiration may exist to a degree of intensity to demand an operation, and yet be so uncertain in their character and results, as to leave us quite in the dark as to the pathological change that had occurred.

On the 21st day after the operation, I caused him to make a full inspiration and then closed the tube, when expiration was performed through the larynx without difficulty and without any noise; he could speak, too with tolerable distinctness; but on

attempting inspiration he seemed to suffer very much; the sound was quite stridulous, and he could not fill his chest (as he said) sufficiently to be comfortable. He also stated that he felt he should be suffocated if the tube was closed for many minutes.

More than* seven weeks have now elapsed, and the patient has been permitted to remain in the hospital for the purpose of observing the farther progress of the case, and seeing whether its nature would become developed by time. As to natural respiration he is pretty nearly in the same state, being perfectly incapable of continuing to respire through the rima even for a few minutes; he breathes comfortably through the tube, and is quite reconciled to the idea of using it for the remainder of his days. He speaks, even without closing the tube, but much more distinctly, and with much more ease to himself, when it is shut up; and it is both interesting and amusing to carry on a conversation with him. When he wishes to speak for any time, he makes a full inspiration through the tube, then closes it and continues to speak whilst that supply of air endures; he then inspires again through the tube, which he again closes, and so on; thus he can continue to speak for a considerable length of time.

This patient (as I have just observed) proves one very interesting physiological fact, not (as far as I know) hitherto sufficiently noticed, namely, that the integrity of the trachea is not necessary to the production of articulate sounds in the larynx. In every instance that I am aware of, *immediately* on the trachea

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* This observation has reference to the time at which the paper was read. He remained in hospital twelve weeks, and has called once every week since; the above description applies very accurately to his condition on the present day, June, 30th, 1836. This is the third of my patients *operated on in the Meath Hospital*, and though breathing through tubes, able to pursue their usual avocations. One operated on November 1829, one December, 1834, and this last, February, 1836.

being wounded, and more particularly if a portion of it is removed, the voice is entirely lost; and although in process of time it returns partially, and the patient is able to speak, yet it requires several weeks before he can articulate with distinctness. The above is an exception to the general rule, and I can adduce another still more remarkable. On the 7th of this month,* a man named Edward Collins, æt. twenty-seven, applied at the Meath Hospital, labouring under great difficulty of breathing, evidently arising from obstruction in the larynx; inspirations made with great effort, and very stridulous; expiration comparatively easy; harsh cough without expectoration; refers all his distress to the chest, and thinks he would obtain relief if he could expectorate; at the same time imagines there is a lump in the throat. This attack of laryngitis, like the former, supervened on fever. The patient had been only out of hospital a few days, and was pale, weak, and emaciated. I need not dwell farther on the symptoms than to state that they increased to such intensity, that on the morning of the 10th, at six o'clock, I was obliged to operate, and I removed a portion of the trachea more than half an inch in length, and from a quarter to a third of an inch in breadth. On the following day, when explaining to a part of the class the operation and its effects, I mentioned a loss of voice among the latter, when, to my great surprise, the man spoke out tolerably clearly, and exclaimed, oh, but I can speak, and better than I did before I was cut.

Having mentioned one very extraordinary deviation from the usual progress of events with respect to the larynx considered as the organ of voice, I may be permitted to allude to another, in which what may be termed its protective sensibility seems to be impaired, or perhaps altogether lost. The use as-

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* This date also refers to April. I may mention here, however, that this patient recovered perfectly: he was able to dispense with the tube on the 3rd May, and left the hospital on the 7th.

cribed to the epiglottis of protecting this organ during the process of deglutition is well known; and it is also known as I have proved by a case already published, that even when this valvular structure is altogether removed, the larynx is still able to defend itself, and the patient swallow both liqids and solids without much, and occasionally without any inconvenience. I confess that to a very recent period I considered the escape of any portion of aliment from a wound in the trachea as a demonstrative proof of the existence of some unnatural communication between the aërial and alimentary tubes, and when such an event has occurred in any or the numerous cases of laryngeal disease that have fallen under my observation, I have not hesitated in pronouncing a very decided opinion. Yet it is a fact that cannot be controverted, that the Wapiti deer, whose larynx was found after death apparently complete in its organization, frequently discharged portions of its food through the aperture that had been made in its windpipe. But not to rely on observations made on the inferior animal, I have been informed by my friend Mr. Adams, that a case has very recently occurred in the Richmond Surgical Hospital, of a young female wounded in the trachea, rather low down in the neck. From this wound portions of the materials attempted to be swallowed escaped, and yet after death the larynx was found healthy, its organization complete, and no unnatural communication whatever between the œsophagus and windpipe in any part or situation whatever.

Having now detailed certain facts connected with laryngeal disease, which I believe to be new, and which at all events are interesting, I proceed to the inductions which they furnish.

It is perfectly evident that an obstruction to the process of respiration may exist to a degree to *demand* the prompt interference of the surgeon, without our being able to calculate on its pathological exciting cause; and this obstruction may continue for a length of time, and the patient subsequently recover as completely as if such lesion of function had never existed,

thereby proving that it was independent of any organic lesion of structure. At the same time it must be confessed, that the majority of subjects thus affected do *not* recover, and therefore in them a lesion of structure may be inferred, although in the present state of our knowledge we may not be able to pronounce accurately upon it. These phenomena I do not profess to be able to explain, but I may be permitted shortly to allude to one or two physiological opinions that might be supposed to bear upon the subject.

The difference of function that has been assigned to the superior and inferior laryngeal nerves is well known, both with respect to voice and respiration. With reference to the latter, it has been supposed that the superior laryngeal branches supplied the muscles which close the glottis—the inferior or recurrent those which opened it: therefore, that if the superior were divided, or their nervous energy otherwise destroyed, the rima would remain open; whilst if the like injury or lesion was inflicted on the recurrents, the respiratory aperture would be fast closed. "After a division of, (says Le Gallois,) and consequent annihilation of nervous energy in the recurrents, every effort to inspire made by the animal only closed the glottis more effectually." This theory, which has very lately been adduced in explanation of the phenomena of the spasmodic croup of children, so admirably described by Dr. Marsh, might, if true, be far more applicable to the disease immediately under consideration; for it is not difficult to conceive that a tumour might compress the recurrent nerves for a given time, increasing progressively, and of course aggravating the patient's distress until the operation became necessary, and then subsiding, when a recovery would, of necessity, ensue. It is painful to be obliged to question a theory so beautiful and apparently so sufficient, but I believe dissection shews that there is not such an isolation in the duties assigned to these nerves, for that the fibriles of both are distributed to both orders of muscles, the dilators and constrictors of the rima. But to set this question at rest as much as possible, (at least to

my own mind,) I endeavoured to repeat some of the experiments on which it rested. With the assistance of Mr. Alcock and Mr. Hart, I had the recurrent nerves of two dogs divided on separate days. In one of them, certainly, a small twig or fibril escaped the knife, and consequently that experiment was incomplete; but in the other there was every reason to believe that the section was properly performed. The result of these experiments, as far as they went, seemed to shew that the function of the recurrents has not been so decidedly ascertained as has been supposed, for most unquestionably their division seemed to have no effect one way or the other on the respiration of the animals. The tone of the howl of one of them seemed to be weaker and more hoarse, which was the only perceptible change effected: and if the presence of two large wounds in the neck, in the immediate neighbourhood of the larynx, and the inflammation thereby produced, be borne in mind, perhaps this change of voice might be otherwise explained. I may mention that the portio dura was after an interval of several days divided in this dog by Mr. Alcock, and *subsequent* to that operation, it panted and breathed with great difficulty after the slightest exercise.*

Having observed in cases of phthisis laryngea, wherein the arytenoid cartilages were expelled in the course of the disease, that previous to such expulsion the difficulty of inspiration was always very great, and that after their removal an amelioration

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* I have allowed this passage to be printed, as it was communicated to the society, but I have since repeated the experiment with a somewhat different result, and believe that a great deal depends on the manner in which it is performed. On the 27th June I divided the recurrents very high up in the neck of a dog, close to the cricoid cartilage, and immediately before their entrance into the larynx. More than an inch in length of each nerve was removed, and in this instance the animal was immediately and completely deprived of voice. There was, however, no observable effect on the respiration. He was made to run about actively for several minutes, but did not pant or give other evidence of a distressed or difficult respiration.

in this respect was clearly perceptible, I have often imagined that any ulceration, any relaxation, or in a word, any lesion of structure which would afford to these bodies too great a degree of mobility, or allow them to be thrown down upon the rima, might possibly explain some of these cases of difficult respiration; particularly those cases in which there was such a remarkable difference between inspiration and expiration. When the connexions between the cricoid and arytenoid cartilages are cut across posteriorly, it certainly is very easy to lay them down in such a manner as nearly to obliterate the rima, and if a similar division be effected by disease, (for abscess and ulceration are of exceedingly frequent occurrence behind the cricoid cartilage,) it seems not improbable that these bodies might become loose, be acted on by the current of air, and shut like a valve in every act of inspiration. I have been informed by Mr. Crampton, that he has seen in the museum of the veterinary college in London, several preparations illustrative of the disease termed "a roarer" in the horse, which seems to be produced by an atrophy of the arytenoid muscles. A relaxation is thus effected which allows to the arytenoid cartilages a degree of unnatural mobility. Whilst the animal is at rest or moving slowly, the current of air passes gently and there is no "roaring"; but when he is put to greater speed and respiration becomes more hurried or more forced, the little valves are acted on, the rima is proportionably closed, the breathing becomes stridulous, and that peculiar noise so well known to persons conversant with horses is produced.

Towards the solution of this difficulty, I offer no hypothesis myself, for I am not prone to speculation on medical subjects, and as yet I have no certain data to proceed upon. Paralysis of the muscles of the larynx has been suggested to me as no *very* inadequate cause of some of the symptoms to which I have alluded; and amongst others, I cannot explain the exit of the food through a wound in the trachea without the existence

of an unnatural communication, on any other supposition. But paralysis is a difficult and delicate subject to touch. We have all seen patients totally and entirely deprived of one function of the larynx, perfectly incapable of uttering any sound, however indistinct and inarticulate, and yet not suffer the slightest inconvenience as to respiration or deglutition. That different nerves minister to these different functions, I am willing to concede; indeed the fact just stated furnishes evidence sufficient on that point; but until we can point out this partition of nervous influence, and more particularly until we can trace their lesions, and the respective effects of these lesions, it will perhaps be better not to found a theory on a basis so very fragile.

I am about to engage in a series of experiments to elucidate some of the subjects already alluded to, which if at all satisfactory in their results I shall be happy to communicate hereafter, as a continuation of this paper.

ART. II.—*Observations suggested by a Comparison of the Post Mortem Appearances produced by Typhous Fever in Dublin, Paris, and Geneva.* By H. C. LOMBARD, Physician to the Geneva Hospital; (communicated in a letter to Dr. Graves.)

Dublin, 16th June, 1836.

MY DEAR FRIEND,

Before I leave Ireland, allow me to express to you my great astonishment at what I have seen in this country respecting your continued fever.

You are well aware of the different views entertained in France and England concerning this important subject; on the one hand the French pathologists and most recent writers, such as Louis, have described the continued fever named typhus, as being always attended by a certain pathological state of the intestinal canal, which begins with swelling and enlargement of

the groups of follicular glands, situated in the lower third of the ileum, and forming the oval patches termed *glandulæ Peyerianæ.* This process is according to them a constant attendant on typhus, and in fatal cases always ends in ulcerations of the mucous membrane The English pathologists, on the other hand, have stated that although they do occasionally meet with the state of mucous membrane described by Louis, yet they do not consider it as being an essential ingredient of fever, and they maintain that fever of a continued and typhous character, is not necessarily connected with any particular morbid appearance or change in the intestinal canal.

Let me observe in the first place, and I beg of you, my dear friend, to attend particularly to this point, that I have now been engaged for more than six years in close attention to this subject, and have seen a great number of fatal examples of typhous fever dissected, but I have never in a single instance missed finding the peculiar state of the intestinal canal above referred to ; amongst other opportunities which I have had of seeing fever, that presented by the epidemic prevalent during the last year at Geneva, and in consequence of which my hospital constantly was supplied with numerous cases of typhus, must not be omitted.

With this experience, and having witnessed numerous dissections of subjects dead of typhus fever, and having found in every one of them at Paris, and at Geneva, the morbid state of the intestinal canal which the French pathologists consider as essential, under these circumstances, when I arrived in Great Britain, and had an opportunity of seeing the fever cases here, and when I found that they presented a very great similarity, if not an identity of symptoms with those I had been for years in the habit of observing, it is not to be wondered at, I say, that I should have expected to find exactly the same *post mortem* appearances. I mentioned this subject to my friends at Glasgow, and they allowed me to dissect the body of a person in whom I said no doubt could exist as to the presence of follicular disease; judge then how great was my astonishment at

not being able to detect a single trace of this morbid change in any part of the intestinal canal, and at finding no marks of disease, save some redness and softness of the mucous membrane of the stomach, which may have been produced by inflammation, but more probably was owing to vascular congestion occurring during the last stage of the disease, or even during the agony which precedes death!

So strong is the influence of preconceived opinions, and so unwilling is the mind to abandon general conclusions formed by an induction from observed facts apparently sufficiently numerous, that although I was much surprised by the result of this dissection, yet I could not bring myself to believe that I had not fallen on an insulated and an extraordinary instance; in short I imagined that I had met with a case rather to be considered as an example of the exception than of the rule.

On my arrival in Dublin, I mentioned this fact to you, and you immediately allowed me to examine the body of a woman who had just died of typhus in the Meath Hopital, and in whose body, from the account you gave of the symptoms and progress of the disease, I had no doubt that I should discover the well known appearances so often observed by me in Geneva and in Paris. Here again I was disappointed, and though I most carefully examined the internal surface of the whole alimentary canal, and examined the mesenteric glands, I could discover no morbid alteration of structure. The physicians of the Hardwicke Fever Hospital supplied me with another opportunity, and the result was similar. In the whole course of my experience I have met with nothing which has surprised me more than this occurrence; I had been for years engaged in the study of typhous fever, and for years my almost daily experience in the dead room led me to associate certain lesions of the alimentary canal with the symptoms of this disease, when suddenly I find myself assailed by a new experience exactly contradictory of my former, nor was my new experience unconfirmed by that of the Glasgow or Dublin physicians. The former assured me that

the morbid changes I had been looking for only occur in about one-third of the bodies examined after death. In Dublin they occur much less frequently during the present epidemic, which has preserved nearly the same characters for the last eighteen months; I have been informed, however, that in former epidemics, as for instance that observed by Dr. Cheyne, and that described by you and Dr. Stokes, they were much more common. This fact rather increases than diminishes the interest of the investigation. You must allow, my dear friend, that such a contrast is well calculated to excite reflections of the most interesting and serious nature, and accordingly you must not deem me tedious in trespassing a little further on your patience. In the first place it is important to bear in mind, for it is the key-stone of my whole reasoning, that I affirm, without fear of contradiction, that the symptoms which in Paris and Geneva I have almost always seen in fever, are exactly those which I have seen in this country in the different fever hospitals that I have visited both in Dublin and Glasgow.

The external appearances are most undoubtedly the same; there is the same headach, pain in the loins, prostration of strength, the same pulse, hot, burning skin, the same depressed expression of countenance, the same furred, dry, parched tongue, and in the latter stages of this disease the same tendency to the formation of bed sores, and to involuntary discharges of stools. The same pulmonary and cerebral complications take place in both fevers, and bronchitis, pneumonia, determination of blood to the head, and anachnitis or at least engorgement of the meninges, occur in both.

Having said wherein consists the general similarity, let me next point out some of the most obvious differences. In the first place then the papular or measles-like eruption which is always found in our continental typhus, but which never acquires any great extent or importance, is here most remarkable, both in appearance and in quantity, for I have seen cases where it formed a rash quite as diffused as that which distinguishes

exanthematous diseases properly so called, and not dissimilar to the papular blotches which cover the skin at the first appearance of the eruption of confluent small-pox; again in your fever this rash is in bad cases mixed with true petechiæ, and in many of the very malignant ones with purple blotches or *vibices*, an occurrence very rare with us.

Let me point out another circumstance, I mean the occurrence of fever in infants and old people; in your hospitals I have seen cases of fever attacking persons who had passed sixty, seventy, or even eighty years of age, and which exhibited the very same appearance, the same typhoid symptoms, and the same eruption that are observed in young and middle aged adults; this observation applies also to infants, for though it has been reported to me that infants at the breast seldom catch fever in Great Britain, yet the contrary undoubtedly sometimes happens, a circumstance I believe never witnessed in France or Switzerland.

Another symptom, which, if I can depend upon what has been reported to me both here and in Glasgow, must be considered as constituting a difference, is diarrhœa, which is much less frequent, both before and during typhous fever, here than it is either in Paris or Geneva. In general indeed it seems to me that the abdominal symptoms are less intense, and of less importance in this country than with us.

Another difference I wish to point out, is the highly contagious nature of the fever of this country compared with that of the continent. I am far from maintaining that the continued fever observed in France and in Geneva is not contagious; on the contrary, I can bring forward undeniable proofs of its transmission by contagion; but yet its transmissibility from one person to another is far less common with us than it is here. In proof of this assertion I may remark, that with us the attendants on the sick are seldom attacked, whereas with you the hospital nurses scarcely ever escape; with us and in Paris, the students who visit the fever wards, the clinical clerks who

take the cases, and the physicians who attend them are not liable to this disease in a greater ratio, or at all events in a much greater ratio, than other persons residing in the same city, and unconnected with any hospital. With you, on the contrary, the very reverse happens, and it is rare to meet with an individual of any of those classes who has escaped being attacked with typhus. So far then the typhus with us differs as to the intensity of its contagion from that endemic in Glasgow and Dublin. In one remarkable point, however, I believe they agree, I mean the fact *that no one is known, or at least is very rarely known, to have the eruptive typhus twice.* With us, such instances are scarcely, if ever, met with, and I am informed that with you, a person once attacked with typhus, attended with the measles-like eruption, may safely calculate upon immunity from this disease for the future, a circumstance which, as has been well remarked by a Glasgow physician, stamps upon typhus one of the distinguishing marks or characters of the exanthemata.

Having thus attempted to present to you in a hasty and very imperfect manner some of the numerous ideas that present themselves to my mind concerning the symptoms which unite and the symptoms which distinguish this fever as observed with you and with us, allow me, my dear friend, to repeat, that I attach much importance to so striking a difference as the *constant* existence of a certain morbid state of the intestines in one case, and its comparatively rare occurrence in the other; a difference the more remarkable, as being unaccompanied by any corresponding difference in the general or external symptoms: I say corresponding, for you perceive that I allow that some difference does exist between the symptoms of your typhus and of ours; but then they are comparatively trifling, and not at all proportioned to the difference that ought to exist, if the state of the alimentary canal plays so prominent a part in producing those symptoms, as we continental physicians have been in the habit of believing. The question here naturally occurs, whether the two

diseases are different or the same? I cannot allow that they are specifically distinct, and consequently I am almost forced to give up the opinion that the local changes of structure are of paramount importance in causing or producing the symptoms that accompany this type of fever. If they be of the importance which many assert, and if the state of the mucous membrane of the intestine, more particularly if the inflammation of Peyer's glands, and their subsequent ulceration, are the chief or the sole cause of the chain of symptoms observed in French typhus, how comes it, I say, that the very same set of symptoms occur in Great Britain, where post mortem investigations place it beyond all doubt that their cause does not reside in a morbid state of the mucous membrane or of Peyer's glands.

All these considerations, my dear friend, seem inevitably to lead to the conclusion, that typhus fever is more a general disease affecting the whole constitution than a malady depending on any local inflammation, or any local change of structure. May we not infer, also, that various causes serve to impress upon this general disease a tendency to associate itself with, and produce various local ailments; among these causes, the most influential probably are, climate, seasons, the race of mankind, diet, and various circumstances which act powerfully both on the mind and body, and which, when concentrated at any one point of time, have given rise to those various epidemics of typhus that have so frequently devastated the different countries of Europe. *This view of the subject must evidently lead to important practical results, for it will, in the first place, make us cautious in applying the treatment applicable in one European country to the typhus of another, for in the latter, local symptoms may exist that render that treatment inapplicable.* Thus the system of active purgation might be pursued with much less injury in Glasgow or in Dublin, than in Paris or Geneva, where the peculiar proneness of the follicular glands to run into ulceration renders the irritation of purgatives a more serious matter.

Again, a knowledge of this fact ought not only to render us more cautious in adopting the practice of foreigners, but also more charitable in criticising either their pathological views or their practical inferences. To conclude, my dear friend, the subject requires a much closer and more accurate investigation than I can now afford time to bestow on it, and I know of no more interesting question which remains to be solved, than what are the circumstances which determine or the symptoms which distinguish the occurrence of the follicular disease in typhus.

I remain yours sincerely,

H. C. S. Lombard.

To Robt. J. Graves, M.D.

Art. III.—*Statistical View of the comparative Frequency in which the principal Medicines used during the last sixty Years have been prescribed in Dublin.* By William D. Moore, Licentiate of Apothecaries' Hall, &c. &c.

Conceiving that it would be interesting, and at the same time useful, to ascertain with precision what medicines have been for some time most relied upon for the removal of disease, and what revolutions each has experienced in the favor of the medical profession, I have made the following analysis of the prescriptions received at the establishment opened by my grandfather in the year 1780, of which accurate copies have been kept, and which afford materials for exhibiting the practice of the most eminent physicians and surgeons of Dublin for a period of nearly sixty years.

In order to effect this, I have divided the entire time into three equal portions; from each of these I have taken 1200 prescriptions, and marked the frequency with which each medicine occurred in them;—the results are shewn in the following

table. The first column contains the years from 1780 to 98, the second terminates with the year 1817, and the third with 1836.—

Emetics,	74	7	6
Enemata,	58	7	20
Blisters,	47	64	39
Warm plasters,	45	10	3
Leeches,	1	20	45

ANTIMONIAL PREPARATIONS.

Tartar emetic,	104	31	50
James's powder,	4	36	44
Kermes mineral,	8	3	0
Antimonial powder,	14	28	15

NARCOTICS AND SEDATIVES.

Opium,	102	79	116
Morphia,	0	0	4
Paregoric elixir,	72	28	13
Digitalis,	0	8	3
Hyoscyamus,	0	4	44
Lactucarium,	0	0	11
Peruvian Bark,	102	42	13
Quinine, sulphate,	0	0	20

SALINE PREPARATIONS.

Alum,	5	1	1
Nitre,	45	9	11
Glauber's salts,	35	12	2
Sal polychrest,	38	0	0
Sulphate of potash,	0	12	22
Rochelle salt,	63	13	22
Epsom salt,	0	213	102

DIAPHORETICS.

Mindererus's spirit,	67	35	22
Nitrate of soda,	3	12	2
——— potash,	6	0	1
——— ammonia,	0	2	21
Guaiacum,	32	10	2
Sarsaparilla,	8	15	5

STIMULANTS, ANTISPASMODICS, &c.

Castor,	8	10	1
Musk,	4	1	0
Assafœtida,	18	7	12
Camphor,	9	22	52
Ammonia,	64	88	39
Ether,	9	22	2
Spirit of nitrous ether,	20	10	10
Iron,	22	42	20

PREPARATIONS OF MERCURY.

Calomel,	84	189	114
Blue pill,	17	39	156
Mercurial ointment,	35	11	1
Ethiop's mineral,	15	0	0
Mercury with chalk, Mercurius alcalizatus,	18	6	35

PURGATIVES AND APERIENTS.

Jalap,	136	35	14
Scammony,	80	120	51
Colocynth,	19	31	67
Aloes,	46	79	63

Senna,	94	107	134
Castor oil,	62	73	32
Rhubarb,	81	59	170
Magnesia,	70	43	52
Syr. rosar. solutio,	21	0	0
EXPECTORANTS, &c.			
Hippo,	62	58	152
Ammoniacum	21	26	11
Squills	33	47	45
Iceland moss,	0	2	7
Basilicon ointment,	32	7	0

From the foregoing table it appears that the emetics which we have met in the first period, have in point of number, to those in the second, the proportion of 10 to 1, while to those in the third, the ratio is 12 to 1. It was at that time the invariable practice to administer an emetic on the accession of an attack of fever, a practice which by no means obtains at present, and which appears to be intimately connected with Dr. Cullen's theory of the proximate cause of fever. In his Practice of Physic published in the year 1777, three years before the commencement of our table, he describes the use of emetics as being " a means of determining to the surface of the body, and thereby taking off the spasm affecting the extreme vessels," and after mentioning their various effects upon the viscera as well as on the surface of the body, he states that they are remedies well suited to the cure of fever. The writer also of the article Emetics in the Edinburgh Medical Dictionary says, that the modern practice of employing emetic remedies in fevers, is founded on their power of producing a degree of action in the stomach, which is communicated to the extreme vessels, so as to restore their tone, and overcome the spasm affecting them. From these and other circumstances, I think I am justified in

concluding that the introduction of emetics into such general use, was at the time to which I have alluded, of recent date, and also in coupling Dr. Cullen's theory of fever with them in their rise and fall.

Enemata occur during the last forty years, taken together, something less than one-half as often as during the first twenty, and it is remarkable, that the period in which they are at their lowest ebb, commences at the time that Dr. Hamilton directed the attention of the medical world to the importance of administering purgative medicines in a great number of diseases. Although he employed enemata frequently in hospital practice, his book appears to me to speak chiefly of purgatives given by the mouth, and I think it not unlikely that the diminution in the use of enemata at that particular time, was mainly owing to the appearance of his work. They seem latterly to have increased again, and are at present extensively used in hospitals.

Local bleeding has been making a gradual and deservedly steady progress, greatly assisted latterly, no doubt, by the attention which Broussais has directed to the state of the gastro-enteric mucous membrane in fever. This increase of its employment may partly account for the diminished frequency of the occurrence of blisters and warm plasters, while the latter have also partly given way to stimulating liniments and other means of counter-iritation.

The great frequency of the occurrence of tartarized antimony in the first period is chiefly owing to its use as an emetic, and as it entered into the composition of almost every one given, at least sixty may be deducted, leaving its ordinary employment at forty-four, so that except as an emetic we meet it most freqently in the third period.

Kermes mineral was formerly much prized as a remedy, though a most violent one, in pneumonia. While it has ceased to be employed, the antimonial and James's powders have increased, and it may be observed, that the total number of the

preparations of antimony is nearly equal in each of the three periods.

The steady manner in which opium has kept its ground, while its camphorated tincture has fallen into comparative disuse, is worthy of remark. The preparations of morphia appear to have displaced the latter rather than the former. It seems to be the opinion of most physicians at present, that they answer sufficiently well as a quieting addition to a cough mixture, but that they cannot, with advantage, supersede the other preparations of opium, when the intention is to procure a night's rest. The black drop is, perhaps, at present the most frequently used for this purpose.

The preponderance of hyoscyamus latterly is due to the extract being now frequently ordered in combination with blue pill, it having become, with many practitioners, the most common corrigent of that medicine.

Previously to the year 1798, Peruvian bark was generally given in the form of powder, and the red was the kind most frequently so prescribed. After that time the infusion and tinctures of the pale bark began to be more used, and latterly the sulphate of quinine has, in a great measure, superseded all.

It will be seen that Epsom salt has been rather recently introduced into general use, its place having been formerly supplied by Rochelle salt, sal polychrest, and the sulphate of soda.

If nitre and James's powder be compared, it will seem as if they had changed places in the favour of the medical profession, at least if we suppose them to be used in similar cases, which I think is not unlikely. This appears at once by placing them together, thus,

Nitre, . . .	45 .	9 .	11
James's powder, . .	4 .	36 .	44
	49	45	55

By the citrates of potash, soda, and ammonia, I mean the solutions obtained by neutralizing the carbonates of those alkalies with lemon juice, as they are generally prescribed in diaphoretic mixtures.

The great increase in the use of blue pill may be attributed to Mr. Abernethy's strong recommendations of it. Indeed, I believe, before his time it was not given except as part of a regular mercurial course, and never in the manner in which it is now so frequently exhibited, namely, as an alterative or antibilious medicine at bed-time, followed or not by a purgative in the morning.

The falling off in the use of mercurial frictions is well shewn in the table by the numbers annexed to the mercurial ointment, a fact to be accounted for by the great improvements in the treatment of the venereal disease.

Jalap has decreased and rhubarb increased more than any other remedies in the class of purgatives and aperients. The syrup of solutive roses, now completely gone out of use, was a frequent adjunct to aperient draughts and mixtures.

Hippo is much more used at present than formerly, and this will appear more clearly, if it be considered that in the first period its use, as well as that of tartarized antimony, was chiefly as an emetic.

It is a curious fact, that fifty years ago blisters were seldom dressed with simple ointment, indeed it appears to have been thought injudicious to allow a blistered surface to heal soon. They were generally either dressed with basalicon, or with the ointment of an indigenous plant called melilot, (trifolium melilotus officinalis of Linnæus,) which I have omitted to mark in the table, although it occurs very frequently as an accompaniment of blisters.

In order to shew the opinions which were entertained with regard to the treatment of blisters, I shall conclude with a quotation from the Complete English Dispensatory, published in the year 1742, where the melilot is described as " consisting of

warm subtle parts, mostly employed in dressing blisters, until they cease running, and skin over, for its warmth still promotes the discharge and prevents mortification, which in some constitutions they are very liable to, especially if dressed with things that are cooling, which the heat of the parts frequently provokes persons *very injudiciously* to venture upon."

Although it very commonly happens that the discharge from the bowels is blackened in consequence of the administration of iron as a medicine, it is very seldom indeed that this metal is introduced into the stomach mixed with food, so as to produce a similar effect. I do not at present recollect any article of diet sufficiently acid, to act on vessels of iron in which it might be prepared, except jelly made in the usual manner, with lemon-juice, &c.; in making this well-tinned vessels are generally employed. From its rarity, therefore, I conceive that such an occurrence as I have alluded to, particularly should it take place during illness or recovery, might prove embarrassing, and were the cause altogether overlooked, even lead to unnecessary treatment, on the supposition that the bowels were in a very deranged state. I shall on this account use the present opportunity to state, that the above effect was, in a case which recently came under my observation, produced by the use of iron vessels in the kitchen. It occurred to a person convalescent from fever, and was at first the source of considerable uneasiness to her, imagining that she was labouring under an accumulation of black bile. When the circumstance was mentioned to me, having ascertained the absence of iron in the water which was used in the house, as I at first suspected there might have been a chalybeate spring in the neighbourhood, I made particular inquiries about the mode in which her food was prepared, and found that some jelly, which she had been ordered as nourishment, had been made in an iron saucepan. On adding to it, when melted, a drop of solution of ferro-cyanate of potash, I obtained a blue precipitate, and was thus enabled at once to set the patient's mind at rest. Although

the portions last poured out appeared, when viewed in a large mass, rather darker than usual, this was not perceptible in looking at it in smaller quantity, nor was any chalybeate taste observed unless when carefully looked for, and even then it could only be perceived by some: in fact, the jelly was sufficiently impregnated to change the colour of the fæces to a deep black, while neither its taste nor appearance had excited the slightest suspicion.

ART. IV.—*Statistics of Fever, with General Observations on its Nature, Causes and Treatment.* By WILLIAM MATEER, M. D., Physician to the Belfast Fever Hospital.

IN the following observations it is intended to point out the various circumstances which have been found to give rise to or to modify fever, as it has occurred in the town of Belfast and its neighbourhood. With the view of ascertaining this, statistical tables have been drawn up from the registry and other documents belonging to the fever hospital in this place, and these exhibit the result of nearly eighteen years' experience of the disease. Latterly, the attention of medical men has been a good deal turned to statistical investigations, and numerical calculation; and they have begun to properly appreciate their value in determining facts in different departments of medical science. Inquiries of this kind have not, however, been so much followed in this country as they might have been, considering the abundance of materials that almost every hospital affords us for this purpose. In all such institutions, records are kept of the age, trade, residence, habits, &c., of the cases admitted into them; and were these arranged in a tabular form, much useful information might be elicited from them. The task of reduction could easily be committed to intelligent patients, confined with trifling ailments, under the superintendance of their medical attendant. In this way the present

tables were procured. They detail all the particulars regarding fever that could rightly be ascertained, or were worth knowing: such as, for example, the amount of deaths and admissions, ratio of mortality, number of cases from each street or locality, of each trade, and the duration of illness both before and after admission. The inferences deducible from these tables refer, in particular, to the causes of fever with which this paper shall be chiefly occupied in considering. We shall, however, also advert to the treatment of this disease, and the principles on which this is founded.

The causes of fever are commonly divided into the exciting and predisposing: but this distinction does not strictly hold, because, they may both assume either shape according to circumstances, and even often concur in their operation. Some of them seem to be specific in their action, and as such may be classed under the head of exciting causes. Of this kind are contagion and atmospheric influence. Fever, as when it occurs in an epidemic form, will originate without any known cause, and then its origin is commonly attributed to certain altered states of the atmosphere. These alterations, whatever they may be, are inappreciable to our senses, or by any means of analysis that we can employ for detecting them: but that some deleterious change has, on such occasions, taken place, almost all writers admit. The way in which atmospheric influnce operates, in giving rise to disease, we have no means of ascertaining. It would seem, at all events, to be independent of the heat and cold of the seasons, as far as we can judge from the occurrence of the disease in this town. Thus, during the eighteen years ending May, 1835, we find admitted into the fever hospital 5078 cases during summer; and 4771 in winter; making a difference of only 307 for such a lengthened period. The following table of quarterly admissions shows the number for each season, and also the general mortality.

From May, 1818, to May, 1835.	Admitted.			Died.			General Mortality for each Season.
	Males.	Females.	Total.	Males.	Females.	Total.	
1st quart. Summer,	1191	1405	2596	82	66	148	1 to $17\frac{27}{37}$ for Summer,
2nd do. Autumn,	1157	1325	2482	79	77	156	1 to $15\frac{71}{78}$ for Autumn.
3rd do. Winter,	1081	1278	2359	79	89	168	1 to $14\frac{1}{24}$ for Winter.
4th do. Spring,	1191	1221	2412	100	7 6	176	1 to $13\frac{31}{44}$ for Spring.
Total Amount,	4620	5229	9849*	340	308	648	

The variability of seasons has more influence in this respect. The years 1826, 1828, and 1831, are stated to have been very changeable, and we find that in these the number of admissions is nearly double that of the years preceding them, as a reference to the general table will show.

Contagion is generally accounted a fruitful source of fever. One main object contemplated in the erection of fever hospitals, was to afford protection to the community from the contagious influence of the disease, by having the sick removed to them. It has, however, been supposed by some, that contagion has not so much to do in the propagation of this and other epidemics as is commonly believed; an opinion which, if corroborated, would go far in lessening the importance of such establishments in the minds of the public. Any one, however, who has attended hospitals, must have found that such a supposition is quite groundless. He will have observed patients being brought in a short time after their friends or acquaintances, or that they are followed by one or more of the remaining members of the family to which they belonged. A table has been drawn up from the registry, with the design of throwing light on this matter. In this table are marked the number of cases which may have had any communication, either by residence in the same house or by belonging to the same family. It has

* This sum includes moribund cases and relapses, amounting to 252, which are not included in the other calculations.

been supposed that when two or more individuals have been brought from the same house or family, the disease has originated in them from contagion, there being communication in these instances, and consequently exposure to infection. The remainder are set down as single cases, or those where no contagion could be traced; though it is obvious from the way in which our calculations on this point are framed, that in many of these this cause may have been in operation, so that the subjoined table does not adduce all the truth in favour of contagion. The table commences with May, 1818, and is carried down to May, 1835, including a period of seventeen years.

Date from May ,, to May	Number of Persons in Families.	Number of Families.	Average No. of Persons to each Family.	Single Cases.	Total.
1818 ,, 1819	905	215	$4\frac{10}{24}$	353	1258
1819 ,, 1820	590	95	$6\frac{4}{19}$	90	680
1820 ,, 1821	680	132	$5\frac{12}{13}$	47	727
1821 ,, 1822	210	65	$3\frac{5}{13}$	49	259
1822 ,, 1823	265	86	$3\frac{1}{12}$	40	305
1823 ,, 1824	185	62	$3\frac{1}{31}$	36	211
1824 ,, 1825	305	95	$3\frac{4}{19}$	103	408
1825 ,, 1826	205	92	$2\frac{7}{30}$	107	312
1826 ,, 1827	701	175	$4\frac{1}{275}$	157	858
1827 ,, 1828	560	138	$3\frac{1}{16}$	196	856
1828 ,, 1829	355	101	$3\frac{26}{50}$	126	481
1829 ,, 1830	130	67	2	25	215
1830 ,, 1831	340	78	$4\frac{4}{3}$	228	568
1831 ,, 1832	705	188	$3\frac{10}{14}$	304	1009
1832 ,, 1833	360	75	$4\frac{12}{13}$	177	537
1833 ,, 1834	315	72	$4\frac{5}{8}$	162	477
1834 ,, 1835	385	120	$3\frac{5}{24}$	252	637
Total	7246	1856	3	2342	9588

It thus appears that the number of families where contagion was traceable is 1856; that the total number of persons belonging to these is 7246, making an average of nearly four individuals to each family; and that the single cases, where the disease seemed to have arisen from other sources, amount only to 2342.

Speedy removal to hospitals ought in all cases to be inculcated, not only for the purpose of checking contagion, but also on account of the patients themselves; for it is found that the danger of a fatal issue to the disease is in exact ratio with the length of time the sick remain in their houses previous to admission. Thus during the seventeen years embraced in this last calculation, and out of 9588 patients admitted in that time, we find the mortality and number of cases on each day of illness as follows :—

Day when admitted.	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	14th	Total.
Number of cases,	544	1081	1669	1106	927	594	1067	302	539	187	247	167	750	9588
Number of deaths,	13	41	82	52	39	40	112	30	57	13	25	7	150	664
Ratio of mortality per cent.	2	3	4	4	4	6	11	10	10	6	10	4	20	

Fractional numbers omitted.

This summary of a more general table, on the same plan, for each separate year, shows us, that 6988 patients were admitted, whose illness before admission ranged from the second to the eighth day, and of this number 375 died, giving a proportion of 5 per cent. of deaths: also that the number of those admitted from the ninth to the fourteenth day, or who had entered on their second week of illness before being brought into the hospital, amounts to 2592 patients, of whom there died 285, leaving a ratio of deaths to the admissions of about 10 per cent. Comparing these results we find that the mortality in the latter instance is exactly double that in the former, and that the deaths are proportionate to the delay in patients being brought into the hospital. This fact is of much importance, and would seem to prove, (for we have no direct evidence in the matter,) that the mortality of those received into the hospital has been much less than it would have been had these same persons been taken care of at their own homes. We do not know the proportion of deaths to recoveries in the latter instance, as no regular account of such is kept by the district attendants. This it would be necessary to have ascertained, so as to form an opinion of the comparative success of medical treatment with patients thus differently situated. In the hospital the mortality varies every year. The general

mortality for the eighteen years ending May, 1835 is $6\frac{7045}{11209}$ per cent. or as 1 to $15\frac{54}{743}$; deducting moribund cases it would be only as 1 to $18\frac{4816}{5967}$.

The predisposing causes of fever are various, such as age, trade, vicious habits of living, innutritious diet, bad ventilation. The most uniform results obtained from any part of our calculations are those in reference to the influence that *age* has on the number and mortality of fever cases. The following condensed table will illustrate this. It includes the period between September, 1817, and May, 1835, during which there have been admitted at the age of

		Cases.	Of which there died.	
From	1 to 5	301	12	Admitted 5214; died 151; being a mortality of $2\frac{2}{2}\frac{3}{6}\frac{6}{0}\frac{6}{7}$ per cent.
	5 to 10	979	13	
	10 to 15	1709	36	
	15 to 20	2225	90	
From	20 to 25	1384	74	Admitted 3747; died 301; or a mortality of $8\frac{1}{3}\frac{2}{7}\frac{4}{4}\frac{4}{7}$ per cent.
	25 to 30	1033	81	
	30 to 35	677	70	
	35 to 40	553	76	
From	40 to 45	418	82	Admitted 1043; died 216; or a mortality of $20\frac{7}{1}\frac{4}{0}\frac{0}{4}\frac{0}{3}$ per cent.
	45 to 50	302	60	
	50 to 55	188	45	
	55 to 60	135	29	
From	60 to 65	86	31	Admitted 171; died 60; being a mortality of $35\frac{1}{1}\frac{5}{7}\frac{5}{1}$ per cent.
	65 to 70	36	12	
	70 to 75	25	11	
	75 to 80	24	6	

By this it would appear that the susceptibility to the disease diminishes, while its fatality increases with the increase in age of the patients. This is best seen by comparing the amount of admissions and of deaths for each successive period of twenty years; the law does not strictly hold in lesser times. It may thus be laid down as a well-established fact, that while the mortality increases,

the number of cases decreases, in proportion to the age, and that the two former (mortality and number) are in relation to this latter in an inverse ratio.

Calculations have been made of the number of cases of fever brought into the hospital from each lane, street, and entry in town, for the purpose of ascertaining the comparatively healthy state of its different localities. The streets inhabeitd by the poorer classes supply the greatest share ; but they differ greatly in this respect. One street called Carrickhill, and its continuation Millfield, with the adjoining lanes and entries, are found to have supplied nearly three-fifths of the whole amount; and yet these are by no means the poorest or worst-ventilated parts of the town. The circumstance would seem to be caused by a deficiency in the proper supply of water to this district, from its occupying a higher elevation than the source whence the rest of the town is supplied. Water is procured in other ways, but with difficulty, and not in the quantities they would otherwise have it. The consequences are want of cleanliness and bad sewerage, so that decayed animal and vegetable matter of all kinds, not being carried off by a current of water in the usual way, accumulate, and so generate miasmata. The atmosphere of such places may in this way be as much contaminated as by improper ventilation.

The influence of trade in giving rise to fever is not so great as we might have expected. It is, however, difficult to arrive at any thing like certainty on this point, because, we do not know the proportion which the different trades bear to the whole population. The following list shows their relative amount in those admitted into the Belfast Fever Hospital from May, 1817, to May, 1835.

Basket-makers, . . .	4	Bricklayers, ...	53
Brush-makers, . . .	15	Brass-founders, . . .	7
Butchers, ...	39	Bonnet-makers, . . .	30
Blacksmiths, ...	51	Bleachers, ...	35
Blue-dyers, ...	26	Bakers, ...	35

Brick-makers,	25	Mendicants,	16
Bookbinders,	12	Nailers,	2
Barbers,	6	Painters,	22
Cotton-spinners,	464	Police-watchmen,	9
Chandlers,	10	Printers,	18
Cutlers,	8	Pilots,	16
Coopers,	59	Paper-makers,	4
Cotton-printers,	49	Pork-cutters,	10
Cotton-warpers,	10	Pensioners,	8
Carpenters,	76	Robers,	31
Curriers,	12	Ragmen,	6
Constabulary-police,	9	Shoemakers,	234
Chimney-sweepers,	8	Servants,	1106
Carmen,	34	Sail-makers,	4
Coach-makers,	10	Semsptresses,	116
Copper-smiths,	12	Sawyers,	50
Clerks,	31	Stonecutters,	25
Cabinet-makers,	12	Sadlers,	11
Dealers,	218	Stucco-workers,	15
Dress-makers,	58	Students,	14
Flax-dressers,	26	Ship-carpenters,	16
Flowerers,	88	Schoolmasters,	20
Founderers,	10	Staymakers,	6
Gardeners,	11	Tobacco-spinners,	35
Glovers,	7	Tanners,	6
Glass-cutters,	6	Tailors,	79
Glass-blowers,	10	Tinsmiths,	12
Hatters,	5	Upholsterers,	5
Hosiers,	11	Weavers,	716
Labourers,	878	Whitesmiths,	10
Lightermen,	13	Washerwomen,	40
Lamp-lighters	4	Watch-makers,	4
Mariners,	131	Turners,	8
Musicians,	23	Various,	66
Mantua-makers,	11		

Total amount of those occupied in trades, . . . 5406

The greater number of patients who have been under treatment in the hospital have had no regular employment; such as mothers of families and children. These amount in all to 5803 individuals. The remainder is chiefly composed of workers out of doors, as labourers, servants, and of weavers, cotton-spinners, shoemakers, who, on the contrary, are employed in close, and for the most part, badly ventilated apartments. More than two-thirds of the total number have belonged to these trades. It is true they are also the most numerous; more individuals belonging to them than to any others, but still their proportion on the fever list is greater than that which they bear to the rest of the population. Exposure to wet and cold, with fatigue from hard working, readily accounts for labourers and such like being liable to be attacked with fever; and again, the sedentary occupation of those who work within doors, their confinement in close rooms, and the anxiety and depression of mind produced by fluctuation in their trade, though circumstances of a somewhat opposite character to those last mentioned, are yet equally powerful agents in causing disease.

Sex would seem to predispose to fever. Females are more susceptible of the disease than males. Thus, of the total number admitted from May, 1818, to May, 1835, inclusive, 5130 we find to have been females, and 4458 males; being a superiority of the former over the latter of 672. This may be accounted for from the circumstance of females being the more numerous part of the population, to their more delicate frame of body, which unfits them for enduring hardships, and particularly also to the custom (almost entirely confined to females of the poorer classes) of going abroad barefooted, by which they are exposed to the rigour and vicissitudes of the seasons. But while the susceptibility to fever is greater with females, the mortality is found to have been less with them than with the males. Taking the general ratio of recoveries to deaths in the admissions of each sex for the period already referred to, we find the recoveries with males to be as $13\frac{25}{31}$ to 1, while with females they are as $16\frac{43}{44}$ to 1.

Such are some of the predisposing and exciting causes from which fever commonly originates; of the intimate nature of the deranged state of the system constituting fever that follows on their action, we are entirely ignorant. There is no one specific symptom characteristic of the disease, nor is there any morbid condition that could justly be considered as essential to its existence; for we do not view in this light local inflammation, as Broussais and others have done. *The disease is a general one, affecting every part of the body, and consists in an altered state of all the vital functions.* The most striking particulars noticeable are, 1st, a more or less regular period of duration; 2ndly, a gradual increase, and afterwards decrease in the intensity of the symptoms; and 3rdly, the occurrence of paroxysms, themselves also exhibiting periods of increase and decrease in their severity, and in this respect being types of the general disease which they compose.

The duration of fever differs according to the species of the complaint, the peculiarity of disposition, mode of living, and many other like causes. From the general table annexed to this paper, it will be found, that the average time for each patient to remain in the hospital under the disease, is about twenty-two days; and this calculation is made from 11209 patients. By the table that details the number of cases admitted on each day of illness, given in another place, it appears that the mean number of days that patients have been ill with fever before being brought to the hospital is about seven, which, added to twenty-two, gives us twenty-nine, as the average number of days required by the disease to run its course, from the time of attack till health is re-established.

During these twenty-nine days, the complaint progresses for a definite period, till a certain point of intensity is attained, after which, if a fatal event have not taken place, the symptoms diminish in severity. This point, commonly termed crisis, occurs at some fixed time. In one hundred consecutive cases, carefully noted for this purpose, it was found, on a mean cal-

culation, that the fourteenth day was that on which the crisis took place, or nearly about one-half the time which, as we have already seen, the whole course of fever takes up in each individual. So that it would seem to be proved by extensive observation, that the disease takes up one-half of twenty-nine days to progress, and the remainder to retrograde, or, in other words, that the periods of increment and that of decrement in the intensity of the symptoms in fever, are equal and proportional. The practical rule to be deduced from this law, is, that no patient ought to be allowed to leave an hospital or his own room, until at least a length of time have elapsed, after a favourable change has taken place, equivalent to that intervening between this latter and the commencement of fever. Otherwise a relapse occurs, and nature being foiled in her work, all has to be done over again; and hence we find, (as one of our tables shews,) that relapses happen on those occasions only where this rule has been broken through. Of 149 relapses occurring from 1818 to 1828, (after which time no account was taken of them in the registry,) the mean duration of the original attack is found to be only twenty-two days, whereas, in other instances, it is twenty-nine. When recovery is taking place there is always felt, on the part of the patient, an eagerness to get home or to leave his bed too soon, which the medical attendants often find difficulty in repressing.

The paroxysms of which the fever seems to be made up, as it were, are so many stages by which it advances in its progress, or retrogades. We have them best seen in the case of remittent and intermittent fevers, but still, by careful observation, we can detect something of the same kind, though masked and often difficult to recognize, in the continued fevers of this country. These almost always assume more or less of the remittent character.

The three particulars, now alluded to, characterize the very extensive class of diseases called fever. This includes a great many varieties or species which it is not necessary here to enu-

merate. The varieties of fever differ from each other in their duration, which is one mode of distinguishing them. Thus we have fever of but one day's duration, hence named ephemeral; another form, called synocha, runs its course in about one fortnight; synochus requires a still longer time, and typhus lasts commonly for three weeks or a month. The different species of continued fever may, for all useful porposes, be classed under the general heads simple and complicated; according as they are free from or accompanied by local inflammation. They may also, in common with all other diseases, be subdivided into the sthenic and asthenic species.

Simple sthenic fever is of very common occurrence, though from the slightness of its symptoms it but rarely falls under hospital treatment. All the remedies required to be made use of are purgatives and occasionally some mild diaphoretic, such as the liquor acet. ammoniæ. Influenza might be classed under this head.

Simple asthenic fever is a form of much more importance. Typhus, adynamic, putrid, and all such fevers belong to this class.* The latter rarely make their appearance now-a-days. They

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* In making this classification, we do so in a very general sense, and only for the purpose of explaining certain points in the treatment of fever. Owing to the degrees in the severity of the disease, and the conversion of its various forms into one another, it is difficult to form any division that would embrace the whole of its varieties. If typhus be considered (as many do) a disease originating in all cases from, and complicated with, local inflammation, the present would be a faulty arrangement. Typhus, however, according to the views of Cullen and others, is of two kinds, the "typhus mitior" and "typhus gravior," the former being free from any inflammation of organs, but accompanied with petechiæ, a tendency to putrescence in the fluids, and great depression in the nervous system. It is this kind of typhus that occurs most frequently in this hospital. As for the typhus "gravior," or that form where organs are inflamed and disorganized, (the mucous membranes in particular,) we have always found it to originate and to progress for a considerable period as synochus. The conversion in the type from this latter to typhus is of very frequent occurrence, and depends on causes to which we shall presently allude.

occur only under the aggravated circumstances of pestilence, famine, long confinement in close apartments. Typhus, however, is of very general occurrence at all times, but particularly among the poor classes, and for obvious reasons. It is asthenic, being accompanied with much debility and want of vital energy. The Greek word τυφω, from which it is derived, is expressive of this, the act of smouldering, so signified, conveying to us a figurative though clear idea of the slow, subdued, undeveloped action which the system exhibits during the course of the disease, contrasting strongly with the active symptoms of inflammatory fever in robust subjects. Typhus being a disease of debility, we are to give stimulants so as to keep up the tone and energy of the system. Of these wine is, perhaps, the best, it is the sheet anchor in this disease, as Sydenham long ago remarked of opium in a similar affection. Much of the efficacy, however, of wine depends on the dose in which we give it, and the time we commence with its use. Incautiously given it may, and often does, prove highly injurious, and gives rise to the very evils we may wish to avoid. The plan I have followed for some time past is to keep from its use as long as possible, and to begin with it only when the pulse has sunk much, and prostration of strength is very decided. This for the most part happens on the tenth or eleventh day of sickness, or on the third or fourth after admission. The next rule to be observed is, that when we have commenced to use it, we are to do so at first in small quantities, and afterwards to increase it gradually till the proper effect is produced. The dose usually given at first to an adult is four ounces. This may be daily increased until, in the course of from three to four days, it is found to amount to twelve or fourteen ounces, which latter is, generally speaking, the "maximum" dose that is advisable to be given. The day on which this quantity falls due to be given is commonly the critical day, or the period when the febrile action has reached its greatest intensity, and thereafter begins to decline. It is easy to see whether the wine so administered has had the proper

effect, by the usual favourable symptoms making their appearance. The patient falls into a sound sleep or becomes quiet, and the countenance itself forms a sufficient index of the favourable change that may have taken place. When this has happened, the large dose of wine is no longer to be administered. *It must be immediately lessened, and that by the same gradation as we increased it, viz.,* two ounces daily, till we come to the quantity we started from, when after a day or two its use may be dispensed with, and a pint of beer given for some time instead. This plan of treatment, together with the regular exhibition of purgatives or laxatives, as occasion would require, and sometimes of tonics during convalescence, is the only one that has been found necessary to adopt for the cure of typhus.

With the precautions referred to it has proved more successful than any other. Otherwise administered, wine comes to be abused, and may do much harm in many cases. It has been sometimes found, that when a large dose of wine was given at the outset of the disease, and soon after admission, the same quantity being continued every day afterwards, that it did not give the expected relief. It then generally produced great irritability, restlessness, wandering of mind, and increase of all the febrile symptoms. These, however, often occur in spite of the most careful management. Relief may, in such cases, be obtained by an anonyne draught; and the mixture that succeeds best in this way, is one composed of the "Mistura Camphoræ," and the "Acet. Morphiæ." By allaying irritability and undue excitement, an anodyne powerfully aids our stimulants. The value of the treatment now pointed out consists in this: that by such means the powers of the animal system are gradually developed, and the quantity of the stimulant is more and more increased according as the system calls for more aid from its increasing depression, and from the curative process requiring more and more vigour, till a certain period (the critical) arrives, when, the object of febrile action having been obtained, and less

strength being required, the stimulant may be gradually withdrawn, and without producing any bad effects.

Other stimulants might be given as "adjuvantia" to wine. Dr. Graves has lately brought into notice the solution of the chloride of soda for this purpose, and he speaks of it in the highest terms, in the case of low typhoid fevers in particular. As inorganic substances supply a more permanent stimulus than those extracted from organic substances, we would be led to expect advantages on these occasions from its use. There are cases of a typhoid character, where, perhaps, wine acts too strongly, and even deleteriously; here the use of the chloride of soda may be indicated. My experience of its powers, however, is too limited to enable me to speak decidedly on the subject.

Complicated Fever, or Synochus, as it is commonly termed, comes next under consideration. Writers speak of two kinds, the "synochus mitior," and the "synochus gravior," answering pretty nearly to the sthenic and asthenic states. Sthenic complicated fever is but of rare occurrence. It seems to be the same as the synocha of Cullen. This disease cannot easily be distinguished from the asthenic form in its first stages. It is only when the disease has progressed somewhat that the characteristic symptoms of the latter make their appearance. The asthenic variety is of more frequent occurrence; and, on account of its being attended with much danger, requires careful watching. Before we treat of the remedies employed for its cure, it may be proper, in order to understand the rationale of their action, to point out some particulars respecting the nature and proximate cause of the disease. We may be able to gain some information on these matters, by comparing typhus with that form now under consideration.

Synochus differs from typhus in its symptoms, being throughout the greater part of the disease of a sthenic nature, exhibiting increased vital action. The difference between these two forms of fever may be attributed, in part, to the degree of the exciting causes; but mainly to peculiarity of habit. Typhus is caused

by those circumstances which excite it, acting on a system *generally* enfeebled and debilitated from the operation of predisposing causes: in the case of synochus we take it to be, that the system similarly acted upon is but *partially* enfeebled,—only one organ is so affected while the rest are comparatively sound. This partial debility arises from various circumstances, such as organization, temperament, mode of living, &c.

The greater number of individuals labour under organic disease of some kind or other. It by no means follows, that persons enjoy an immunity from such because they are apparently healthy; for it often happens with these, when suddenly cut off by accidents, that extensive disorganization has been found to exist in some one or more organs. Organic changes are very gradual in their development. They creep on silently, and so often escape our notice. The kind of organ that thus becomes deranged during apparent health, or suffers with inflammation in an attack of fever, is determined by various circumstances,—by organization, many being born with peculiar susceptibilities in certain parts, owing to increased sensibility or other causes. The most important agents are, trade, age, habits of living. Innutritious diet, during childhood, may give rise to worms, and these, by irritating the intestinal canal, may dispose in after life such parts to take on diseased action. Various trades, such as cutlers, knife-grinders, stone-cutters, masons, cotton and flax spinners, from inhaling particles floating in the atmosphere of their workshops, have their lungs kept in a constant state of irritation; and these very often labour under thoracic complaints. In the same way, too, nervous affections and determination to the head are induced, by anxiety, depression of mind, and close study or application of any kind. Peculiarity of temperament operates also in this way; and, by these and other such means, there is produced a weakened condition of particular organs, which are in consequence sure to become affected with chronic complaints at some period of life, or with local inflammation if the system be attacked with fever. It ha

been frequently observed, that, when a patient under treatment in the hospital for a chronic ailment became infected with fever, (as often happened, from the wards appropriated to these being close adjoining to the fever apartments,*) the particular organ, cerebral, thoracic, or abdominal, which had previously suffered from disease became the seat of local inflammation during the febrile attack.

Particular organs being thus enfeebled, when the exciting causes of fever operate, and febrile action is established, they will form an impediment, as it were, to the proper cooperation of the rest of the functions. A general call being made on the system to repair the injury received, the circle of functions cannot be brought properly into play from the failure of a part. This circumstance will change the character of the disease, not only rendering it of longer continuance, but also by giving rise to symptoms of local inflammation. The circulating and capillary vessels of the diseased organ, being unable to transmit the blood and other fluids through them as quickly as the other parts do, there will ensue a "stasis,"—an accumulation or congestion in them, thus causing what is commonly supposed by medical writers to be inflammation of the organ in which it occurs. The name inflammation, as so applied, is not objectionable: rather, if modern doctrines on the subject be correct, the state of the organs complicated in fever is really one of inflammation. According to some physiologists, inflammation ought to be viewed as resulting from a passive rather than an active condition of the blood vessels and capillaries.† It has been supposed to be caused by the passive distention of the vessels of the diseased part by their contained fluids, owing to want of tone in their contractile coats. The vessels in inflamed

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* From the general Table it appears, that there have, in all, been forty-four such instances.

† See Dr. Alison's opinion, as given in the Transactions of the last meeting of the British Association, *Dublin Medical Journal* for September 1835.

parts became relaxed and weakened, and having less power of contracting on their contents, so as to propel them, the circulation is retarded. At the same time, too, there is an increased movement in the surrounding ones; so that inflammation is only another name for morbid congestion of parts. The condition now spoken of seems to be that of the organs which suffer in synochus. They are in a debilitated state, and unable to cooperate with the rest of the functions in many respects, but visibly in one, that of forwarding the fluids through them, so that these stagnate, and thus give rise to inflammation. We have then only to suppose that there had previously existed a weakened state of some organ, and then all the phenomena occurring in complicated fever are easily explained. But synochus, notwithstanding this partial debility, is sthenic at its outset. This may be accounted for by there not existing general debility, as in the case of typhus; and also from the circumstance itself of some one or more of the organs being inflamed. It is a law of physiology, that when a part is inflamed, those neighbouring to it are brought into an opposite state by a kind of *induction*, as it were. And this is a wise provision of the animal economy; for as inflammation consists in local weakness, producing congestion, this is only to be overcome and relieved by an increased vital energy in the surrounding parts. The debility is in this way relieved. An organ being inflamed in the course of fever, there is a call made on the rest of the system to remove the impediment, and thus to produce that equilibrium, which it is the object of febrile action to bring about, and in which health consists. When synochus has continued for some time sthenic, it then becomes of a completely opposite character, after inflammation has been subdued by medical treatment or by nature's own salutary efforts. The system becomes depressed in a degree corresponding to the previous excitement, and thus by another law of the animal economy, that extreme action is always followed by depression. We must bear this in mind, in order to account for our modes of treatment in this disease,

which, according to these states may exist, will be very opposite. In the first stage our object is to keep in check the local inflammation; this may be done by the use of antimonials and squills in chest affections, of chalk mixture, Dover's powder, with the hydrargyr. c. creta and astringents in abdominal complications, and when the head suffers by the application to it of blisters, emollient plasters and cold lotions. Unless, however, the inflammatory symptoms be very urgent, and that we fear disorganization, we should interfere as little as possible, at least, as far as depletory measures are concerned, and act rather the part of the expectant. We are to bear in mind that the decided treatment necessary for subduing inflammatory action, whatever relief it may give for the time, tells but too forcibly when (to use phrases employed by Andral) the hyperemic give place to the anemic symptoms. Though we do not object to the name inflammation, as applied to the diseased state of organs complicated in synochus, we do, decidedly so, to the treatment that some practitioners, on the supposed analogy of these pathological states, would have us to follow: bleeding and other antiphlogistic remedies, in a disease exhibiting such opposite forms, will generally do harm. This mode of cure may bear us off a while from Scylla to be afterwards dragged into Charybdis. The only part of the treatment called antiphlogistic that has been found useful, is blistering. The application of a blister to the head, chest, or abdomen, acts beneficially by stimulating the subjacent organs. With the same view too we may apply, in less severe cases, sinapisms and turpentine epithems. The stimulus these supply, rouses into action the internal organs, and enables them to remove congestion. Blisters to the head are a very salutary measure when any affection of this part exists. Even in doubtful cases, where the symptoms are but slight, they may be applied with safety, for if their use on these occasions, on account of the symptoms being deceptive, is found to have been uncalled for, they will nevertheless have done no injury. When the condition of the

patient is more alarming, when we find a dull expression of the eye, injected conjunctiva, throbbing of the arteries, and increased heat of the scalp, with more or less of delirium, blistering gives manifest relief; and in these instances, it has been often noticed, that the vesicated surface remains open and suppurates freely till the head symptoms have disappeared, and convalescence is established. Blisters act by stimulating, as we have just now said, the vessels of the parts beneath which they are applied; and when, as in the inflamed state of the viscera, there is congestion of the fluids within their vessels, these are by such means enabled to propel them, or the organs have their secretory functions revived; and farther, if there be any tendency to morbid disorganization, it is averted, and the new suppurative action produced externally takes the place of, and supplants the diseased one in the interior.

The local inflammation subsiding, and being followed as a consequence by great weakness, so great sometimes, that this form of fever at this period simulates typhus, we are now to treat our patient as if the complaint were in reality from its commencement one of a typhoid kind. We are to give stimulants and tonics, and from their use the best results follow, provided they have been judiciously administered. In many cases it will not be necessary to employ any remedies whatever, the system of itself (if it have not been unduly weakened by active depletory measures,) rapidly regaining its former healthy condition.

When individuals, in no way debilitated, and free from organic defects, become the subjects of fever, it then proves very mild. Febrile action runs high, but is of short duration; of this kind are the less dangerous forms, such as ephemeral fever, synocha, or the synochus mitior of some authors. Their treatment has been already referred to. It is very simple, owing to there being no general debility, or any visceral complication as we find in typhus and synochus.

A TABLE,

Shewing the Number of Fever Patients admitted into the Belfast Fever Hospital, the Amount of Deaths, Moribund Cases, the Ratio of Mortality, General and Annual, from May, 1817, to May, 1835; with an Account of the Expenditure for the Ten Years, ending May, 1833.

DATE. From „ to	ADMITTED.			DIED.			Moribund Cases.	Cases combined with Chronic Diseases.	Total.	General Ratio of Deaths to the Recoveries.	Ratio of Deaths deducting Moribund Cases.	RELAPSES.		Average No. of days for each Patient to remain in Hospital.	EXPENDITURE FOR 10 YEARS.			ANNUAL AVERAGE EXPENDITURE FOR		
May „ May	Males	Fem.	Total.	Males	Fem.	Total						Adm.	Died.							
1817 „ 1818	„	„	1621	„	„	79	18	„	18	1 to $20\frac{40}{79}$	1 to $26\frac{340}{61}$	„	„	25	£	s.	d.	Wine,		
1818 „ 1819	520	738	1258	20	42	62	7	1	8	„ $20\frac{18}{62}$	„ $23\frac{16}{54}$	49	1	25	„	„	„	£	s.	d.
1819 „ 1820	311	369	680	17	24	41	„	13	13	„ $16\frac{24}{41}$	„ $24\frac{8}{28}$	43	0	22	„	„	„	85	16	4
1820 „ 1821	368	359	727	29	12	41	9	„	9	„ $17\frac{12}{19}$	„ $22\frac{13}{32}$	31	0	24	„	„	„	Medicines,		
1821 „ 1822	124	135	259	14	5	19	11	„	11	„ $13\frac{8}{27}$	„ $32\frac{3}{8}$	0	0	23	„	„	„	185	2	$0\frac{1}{2}$
1822 „ 1823	148	157	305	15	12	27	3	2	5	„ $11\frac{4}{24}$	„ $13\frac{18}{22}$	1	0	23	„	„	„	Food,		
1823 „ 1824	115	96	211	10	13	23	1	„	1	„ 9	„ $9\frac{13}{22}$	3	1	21	1247	11	$0\frac{1}{2}$	650	0	7
1824 „ 1825	182	226	408	14	11	25	3	4	7	„ $16\frac{8}{25}$	„ $22\frac{12}{18}$	7	0	21	1265	9	0	Furniture,		
1825 „ 1826	143	169	312	10	9	19	5	3	8	„ $16\frac{8}{19}$	„ $28\frac{4}{11}$	2	0	25	1108	8	8	Clothing, &c.		
1826 „ 1827	391	467	858	28	32	60	9	7	16	„ $14\frac{28}{60}$	„ $19\frac{22}{44}$	8	1	24	„	„	„	92	10	$0\frac{1}{2}$
1827 „ 1828	299	357	656	20	16	36	6	7	13	„ $18\frac{8}{35}$	„ $28\frac{12}{23}$	4	0	22	1546	15	0	Coals, Soap,		
1828 „ 1829	219	262	481	8	12	20	1	„	1	„ $24\frac{1}{20}$	„ $25\frac{6}{19}$	25	1692	10	4	and Candles,				
1829 „ 1830	103	102	205	7	3	10	2	„	2	„ $20\frac{5}{10}$	„ $23\frac{5}{8}$	25	1373	14	$3\frac{1}{2}$	90	2	7		
1830 „ 1831	258	310	568	21	26	47	1	3	4	„ $12\frac{4}{47}$	„ $13\frac{8}{43}$	25	1410	7	7	Salaries,				
1831 „ 1832	479	530	1009	44	35	79	4	2	6	„ $12\frac{61}{79}$	„ $13\frac{60}{73}$	22	1639	1	$11\frac{1}{2}$	150	16	$3\frac{1}{2}$		
1832 „ 1833	268	269	537	30	22	52	11	1	12	„ $10\frac{17}{52}$	„ $14\frac{17}{40}$	20	1604	14	$5\frac{1}{2}$					
1833 „ 1834	229	248	477	19	24	43	9	„	9	„ $11\frac{4}{43}$	„ $14\frac{1}{34}$	20	1467	11	$3\frac{1}{2}$	Various,				
1834 „ 1835	301	336	637	31	29	60	3	1	4	„ $10\frac{27}{60}$	„ $11\frac{21}{54}$	21	„	„	„	153	5	11		
Total Amt.	4458	5130	11209	337	327	743	103	44	147	1 „ $15\frac{54}{743}$	1 „ $18\frac{481}{596}$	149	3	$22\frac{3510}{11209}$	14077	12	$2\frac{1}{2}$	1407	15	$3\frac{1}{2}$

No account of the Relapses taken after this date.

Fractional Numbers omitted.

Art. V.—*Medical Problems.* By William Griffin, M.D., Limerick.

Continued from Vol. VI. p. 401.

How are neuralgic affections to be distinguished from inflammatory?

In treating of peritoneal and enteric inflammations, Dr. Abercrombie states as conclusions from facts, "that extensive and highly dangerous inflammation may exist in the intestinal canal, without obstruction of the bowels, and it may go on to a fatal termination, while these are in a natural state or easily regulated by mild medicines through the whole course of the disease."

"That extensive and fatal inflammation may be going on with every variety in the pulse; it may be frequent and small, it may be frequent and full, or it may be little above the natural standard through the whole course of the disease."

"That extensive inflammation may go on without vomiting and without constant pain; the pain often occurring in paroxysms, and leaving long intervals of comparative ease."

"Keeping in view these sources of uncertainty," he says, "our chief reliance for the diagnosis of this important class of diseases, must be on the tenderness of the abdomen. This symptom should always be watched with the most anxious care, *whatever may be the state of the bowels or of the pulse, or the actual complaint of pain,* and though the tenderness itself should be limited to a defined space of no great extent."

Whatever the experience of the profession may have determined with respect to the greater part of these conclusions before the appearance of Dr. Abercrombie's work, I believe it has been always in accordance with the opinions of that distinguished physician on the importance which should be attached to the presence or absence of abdominal tenderness in cases of

suspected inflammation, but Dr. Abercrombie was very well aware, when he gave such just weight to this symptom as a means of diagnosis, that in itself it was far from conclusive, and derived its value chiefly from the existence of many other corroborating signs. He states indeed distinctly in another part of his work, "that pain increased upon pressure does not appear to be a certain mark of inflammation in the bowels, for it occurred in Case XXIV., (related by him,) in which there was no inflammation; and in several other cases, it was met with before probably inflammation had commenced. From various observations (he states) he is satisfied, that intestine which has become rapidly distended is painful upon pressure; it is, however, a kind of pain which, *by attention, can generally* be distinguished from the acute tenderness of peritonitis."

This point of diagnosis applies altogether to the discrimination of cases of ileus from those of inflammation, and even as such, is, it would appear, often of doubtful value; but in what light must we view it, in reference to those frequent neuralgic affections, which, whether hysterical or the results of irritation of the spinal cord, are often established suddenly, with little preparatory disorder, and with no distention of the intestinal canal, to account for the acute tenderness on pressure? No medical man is now ignorant of the fact, that the contents of both the thorax and abdomen, as well as their parietes, are subject to attacks of a violent pain of a nervous or spasmodic character, yet with acute soreness to the touch, as in pure inflammations. These are so widely separated from the latter in their nature, and require such a very different, I might almost say opposite mode of treatment, that a correct diagnosis becomes a matter of still greater interest than in cases of ileus. There are few instances of ileus in which one effective bleeding might not be of effective service, while there are few of hysterical pseudo-peritonitis, or enteritis, or of similar affections arising from irritation of the cord, which might not be made worse, or indefinitely protracted by it—yet we find in our best elementary

works on this subject a most perplexing indefiniteness, a diffuseness of description, and a labouring at discrimination, wholly unworthy of the present improved state of medical science, and unnecessary, if any real or essential difference of character could be pointed out. It is indeed, because there is an essential agreement in all material points, that our attention is directed to the attitude, the expression of countenance, the manner of complaining, and even the temper of patients, although there must be sometimes very considerable differences as to all these in individuals similarly affected, and in any case, no slight degree of experience is required to form a proper estimate of their value. We are told that one affection is frequent, the other rare in comparison, that females are more liable to it than the other sex, and those of sedentary habits in towns, than those leading active lives in the country, all of which might be very useful information, so far as it tended to corroborate features of a more marked expression, but is essentially loose and vague, if considered by itself. Again we are reminded of one or two truisms, that if a disease has lasted long and done no mischief, or if it has been aggravated or not aggravated by an antiphlogistic plan of treatment, its character is not inflammatory. We may thus arrive at a correct diagnosis about the time the complaint ought to be cured and forgotten. But the climax of our difficulties on this subject is displayed in the admissions of experienced physicians, that their practice, when in doubt, is to run the risk of erring on the safe side, and that the disease is inflammatory;—perhaps a judicious plan enough under the circumstances, but certainly not so consolotary to the patient, nor as I have elsewhere remarked,* very creditable to medical science. It may be questioned too, whether a rule of the kind with young physicians would lead only to errors on the safe side. I have seen one valuable life lost, and others endangered by such practice, and sometimes the antiphlogistic treat-

* See Treatise on Functional Affections of the Spinal Cord.

ment vigorously adopted, not indeed where doubts existed in the mind of the practitioner, but where the cases were altogether mistaken.

I am far from assuming to myself a perfect freedom from perplexity in all possible cases simulating inflammations of internal organs which may come before me, but with the facts I have stated impressed on my mind, I cannot but feel that the experienced physician is usually too well satisfied with that indefinable power of recognizing or identifying diseases almost unconsciously acquired in the course of long practice. In reflecting on the melancholy steps by which it is attained, on the sad, though perhaps excusable errors committed, he consoles himself with the conviction, that such experience only could gift him with a knowledge which neither books nor lectures had taught him, when he should rather feel, that the true reparation to his conscience, the real duty he had to perform in acquittance, was to prevent the occurrence of such mistakes with others, by analyzing the characters of those perplexing diseases, and endeavouring to trace the sources from which his late discrimination was derived. It is not enough for a practitioner in a difficult case to be assured he is himself capable of determining its nature; he should consider whether his knowledge or ability admits of being communicated to others, and when he believes it is, but not until then, he may also believe that he has acquired information of infinitely greater value to the public, than any thing it could have suffered from his early mistakes.

I have been led to indulge in these observations in consequence of the apparent inattention with which a suggestion with respect to the diagnosis of neuralgic affections, proposed by me some years since was received by the profession: I then stated, that in any doubtful cases I believed if tenderness on pressure *at the portion of the spinal column corresponding with the disturbed organ* existed, it might be considered decidedly neuralgic, but if no such symptom was found, it was probably inflammatory. The suggestion was then offered rather

as a result of individual observation, which I was anxious should be tested by the universal experience of the profession, with a view of claiming attention for an incontrovertible fact; but I have since so repeatedly derived ready assistance in simulated inflammation, by assuming it as such, and have had the diagnosis so invariably borne out by the result, that I do not now hesitate to assert, it is almost the only single symptom upon which a young practitioner can rely without danger.

Without entering into the question of the nature of spinal irritation, which I have discussed at large in another place, I may be permitted to claim the reader's attention to two points connected with it, which must be regarded as physiological facts. That the spinal cord, as the experiments of Le Gallois have shown, is composed of portions independent of one another in their powers and functions, being centres from which the nervous actions of corresponding parts of the body emanate, and to which they tend; and, secondly, in conformance with the well known law, by which the pain and tenderness arising from disorder at the origin or trunk of a nerve, is referred to, or felt at its extremity, that affections of the spinal cord are not usually recognizable by pain at the part diseased, but at the terminations of the nerves in distant organs arising from it. From these facts there is one undeniable inference in determining the diagnosis of cases resembling inflammation,—that wherever spinal tenderness exists, we must at all events set down pain and tenderness (the two most important symptoms in assisting us to detect internal inflammation) as wholly valueless; inasmuch as, whether there be inflammation or not, these are not peculiarly the results of it, but may arise also from the tender state of the cord. I might perhaps go much farther, and assume, that where spinal tenderness exists, there also exists a state of the system scarcely compatible with acute inflammation.

If then pain and soreness on pressure, those supposed

characteristics of inflammation, are of all others the most equivocal symptoms, belong as certainly to irritative as to inflammatory affections, and almost necessarily existing if there be acute spinal tenderness; what is to be our guide in deciding the diagnosis when called to a person suffering with violent pain of side, feverishness, difficulty of respiration, and soreness of the intercostal muscles; or with violent pain in the abdomen, accompanied by exquisite tenderness to the touch, and perhaps constipation and vomiting—*if we do not examine the spinal column?* We want to ascertain the simple fact in the first instance, of whether the pain is in the viscus supposed to be affected at all, or whether it be merely in the thoracic or abdominal parietes. And since the patient shrinks and complains on pressure in both cases, what, I ask again, is to be our guide, *if we do not examine the spinal column?* Are we to depend upon speculations on the attitude, expression of countenance, temper or manner of a patient, when we can at once lay a finger on the spine, and detect both the cause and nature of his complaint.

If in the former case we find acute tenderness of some of the dorsal vertebræ, or in the latter of some of the lumbar, I do not hesitate to say, the one is not pleuritis, nor the other enteritis, nor will either bear large depletion with impunity. The pain and tenderness are merely referred to the extremities of the spinal nerves, ramified through the intercostals or abdominal muscles, from the affection of the corresponding portion of the spinal column, and indicate nothing whatsoever of the state of the viscera internal to them, which usually excites so much alarm. If these are facts, and I believe few will deny them to be so, on examining for themselves, is it not absolutely leading the young practitioner into those mistakes which we so much deprecate, to associate pain and tenderness on pressure so exclusively in his imagination with inflammatory diseases? Is it not strange too, that cases are every day published in our periodicals with dissertations on the difficulty of

their diagnosis, without the slightest allusion to the spinal cord, the state of which, in every case of presumed inflammation, our present knowledge of physiology must shew us the necessity of ascertaining.* One would suppose where the obscurity of the diagnosis is often so undeniable, that any adventitious light which could be brought to bear upon it would be sought for with avidity, but this, every day's experience assures us, is very far from the reality. As illustration is very generally more impressive than argument, I shall offer two or three cases to the consideration of the reader.

A young woman aged 25 years, was attacked with pain in the bowels at night after a feeling of chilliness. She took some essence of peppermint and went to bed, but the pain gradually increased, and at 2 o'clock in the morning, she took twenty drops of laudanum. At 7 o'clock, the pain still continuing, she

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* There was a paper from Dr. Samuel Cusack in one of last year's Dublin Medical Journals, "On certain Nervous Affections occurring principally in Females." It was an essay in fact on the treatment and diagnosis of those chronic pains below the mammæ, or between the false ribs and ileum, so often mistaken for phthisis, pleurisy, liver, complaint, &c. to which public attention has been directed of late years by several writers, as instances of spinal irritation. Dr. Marshal Hall described them long since in his work on diagnosis, and more at large in his work on the diseases of females. Mr. R. P. Player published a letter on the subject as long ago as 1821. Dr. Brown of Glasgow published precisely similar cases in 1828, demonstrating the connexion of the pain with the spinal cord, and since then we have had essays from Dr. Darwall of Birmingham; Mr. Teale of Leeds; and Dr. Marshall of Manchester, all pointing out the nature of those pains and the little connexion they had with a disease of the viscera apparently affected. It is of course open to any physician to publish cases and observations in support of particular views, however frequently placed before the public already by others; but it seems singular Dr. Cusack should conceive it necessary in 1834 to publish an essay on these affections, not only without the slightest allusion to their possible connexion with the state of the spinal cord, but without the least mention of any of those writers, to whom we are indebted for both the descriptions which he gives, and a diagnosis so much more accurate than his, in its not omitting our examination of the vertebræ.

took castor oil, with ten or fifteen drops more of the laudanum, by the directions of an apothecary, and soon after ten drops were repeated in a saline draught. I saw her at 1 o'clock, and found her writhing with pain, chiefly round the umbilicus and to the right side. It became more violent by fits like colic, though never entirely subsiding, and during the intervals of comparative relief, she sometimes threw her arms about restlessly, sometimes lay as if insensible, with the eyes turned up and the lids half open. Her complaints were low, scarcely audible, her respiration painful when deep; turning from side to side increased her pain, although when the paroxysm occurred she turned sometimes on her face. There was excessive tenderness at the abdomen; the least pressure making her scream. She had been constantly vomiting for the last few hours. The castor oil had operated once scantily, her pulse was but little quickened, and there was no heat of skin.

Here was a case of constant pain in the abdomen chiefly about the umbilical region, liable to severe exacerbations, attended by exquisite tenderness on pressure, vomiting and constipation, and continuing for twenty-four hours. I am convinced that almost any young physician would have felt great difficulty, indeed almost an impossibility of determining from a consideration of the symptoms, that the complaint *was not inflammatory*, and I believe that the great majority of either young or experienced ones would at once infer the existence of inflammation, and bleed. I say so from having witnessed it, and from having early in my own professional life always prescribed in such cases with timidity, as if I felt that all consideration of the symptoms led to little better than conjecture. I had now, however, new ground for a diagnosis in the state of the spinal column, on examining which my mind was set at rest. As soon as I pressed on the upper lumbar vertebræ the girl started violently, caught my hand, and complained that I had hurt her dreadfully: the pressure, she said, had increased the abdominal pain; she had never had any hysterical attack in her

life. I ordered the abdomen to be fomented freely, and gave her five grains of calomel with half a grain of opium, directing five grains of aloes and five of extract of henbane, to be taken every two hours. The calomel and the first dose of the aloetic pills remained on the stomach, but the succeeding doses were rejected; a purgative draught was also thrown off; she then got a purgative enema, which was repeated in an hour, but both passed off without any appearance of fæces. The vomiting, pain, and tenderness of abdomen, continued very severely throughout the evening. At night she got a turpentine enema, and was placed in a hot bath which gave some relief, and procured a scanty evacuation. The pain, however, soon recurred, and she was ordered a grain of opium, to be repeated every hour until it should subside. The bowels were freely moved soon after taking the first dose, but she did not experience any considerable relief until she had taken the third, after which she fell into a sound sleep, and in the morning was in every respect improved. The pain had altogether subsided, and the exquisite tenderness was now felt in the right epigastric region only. She had threatenings of a return of the attack in the course of the day, but it was readily subdued by a repetition of the opiate. On the following morning there was scarcely any pain or tenderness, and if the complaint had been inflammatory, I would now have left the patient's bowels perfectly at rest. But believing it to be an affection of the spinal cord, arising from disorder of the digestive functions, I ordered another dose of castor oil, which operated freely, and was followed by no recurrence of the attack.

Mrs.—— aged 30 years, of a full habit, complained of pain in the right iliac region, which gradually increased until the whole of the abdomen became sore to the touch; on the third day she took castor oil, which operated freely, but gave no relief. I saw her on the fifth when she complained of being much worse, and directed her to get to bed, to have the bowels fomented, and to take pills of aloes and henbane every second

hour until relief was obtained. In the morning I received a letter, stating, that she had been in agony all night, was much swelled, had not had her bowels moved for three days, and was now feverish and throwing every thing off her stomach. I became excessively alarmed, and regretted I had not bled her on the night before. On arriving at her residence I found her complaining of violent pain all over the abdomen, but most acutely in the right iliac region, and almost down to the pubes at that side. There was some fulness of the bowels, and the greatest tenderness on pressure. She could not bear the slightest touch in the iliac region, and one spot she described as exquisitely sore. It was on this account, perhaps, that she bore the weight of my hand laid flat, and pressed very gradually, better than pressure with the points of the fingers, as in the former case the pressure on the most painful spot was not so direct. Her pulse was ninety-six, her skin feverish, and tongue white; she had nausea and occasional vomiting, and could not turn or move in the bed, without considerably increasing her sufferings. The only symptom that could lead me for a moment to doubt, that I had here a case of very serious inflammation to deal with, was the expression of the countenance, which did not betray the deep anxiety and distress I have usually seen in the inflammation of vital organs, although her own description of her suffering was sufficiently alarming.

The treatment of the case with decision and confidence now depended altogether on the examination of the spine. If I found a state of increased sensibility and excitement of that part of the cord which gave origin to the lumbar nerves, the pain and tenderness of abdomen would be accounted for, and I need not infer a more alarming cause for them. If, on the other hand, there was no spinal tenderness, the existence of acute inflammation was almost certain, and the most active treatment was demanded. On pressing the spinal column as soon as I reached the lumbar vertebræ, she started and screamed aloud. She also felt excessive pain on my touching the

sacrum, or making pressure behind the trochanter, or at the front of the hip joint in the groin. I therefore unhesitatingly concluded, it was one of those nervous affections simulating inflammation, in which the progress of the case fully bore me out. A few leeches were applied to the most painful part of the abdomen, and a grain of opium was directed every second or third hour until there was some abatement of pain; she was also directed as soon as the vomiting abated to resume the pills of aloes and henbane, and take a dose of castor oil. She got considerable relief, but passed a restless night. In the morning she took the aperient medicine, which operated freely, and now had pain occasionally only, and at long intervals; the vomiting had ceased, but the soreness of the abdomen was still exquisite in the right ileum and groin, and she screamed when I pressed on the vertebra or immediately behind the trochanter. Fomentations were now directed; belladonna plasters were afterwards laid over the painful parts, and the opium pills were given every fifth or sixth hour. She spent the day well, but the pain recurred in the night very severely, and was relieved by the anodynes. Next morning the bowels were again moved, but the soreness on pressure or on moving much in the bed still continued. The case went on in this way for several days, abating very gradually, but the soreness of the abdomen and of the hip joint was not perfectly removed until she was for some days driving out in her carriage.

I attended this lady afterwards for an attack of acute bronchitis, which was of a similar character in its advanced stages. The cough, oppression, and expectoration, were evidently the result of nervous irritation, and required tonics rather than the continuance of any antiphlogistic treatment. She told me that in a former illness, which she called inflammation of the lungs, the doctor had bled and I believe blistered her more than once, that she was going on from bad to worse with incessant cough and debility, until at last, in defiance of all advice, she took a glass of pure wine, which cured her like a charm. I

mention this, not that one can infer much from such loose statements, but because it gives some idea of the irritability of her nervous system.

Mrs. ——— of a delicate, nervous habit, after a natural labour, had internal hæmorrhage, with pallid countenance, cold clammy sweats, chilliness, and an almost imperceptible pulse at 156; the hæmorrhage ceased on the extraction of several clots with the hand, but the debility continued to an alarming degree for several hours. She had large doses of laudanum, with dilute sulphuric acid, through the night, which seemed to relieve her much; but in the morning the debility was still great, the pulse weak at 130, the features sunk, and the respiration much hurried. On the following evening she had a severe rigor, and was soon after attacked with pain and tenderness in the uterine region, excessive pain in the head, brow, and eyes, with sickness of stomach and vomiting; there were thirst and heat of skin, and the pulse became rather hard at 140; the tenderness on pressing over the uterus was considerable, the pain constant, the lochia diminished. On examining the spine, there was found acute tenderness of the lumbar vertebræ, upon which it was assumed to be a case of nervous irritation and not inflammation. A dose of calomel with extract of henbane was given, fomentations were applied to the lower part of the abdomen, and she got diaphoretics at intervals. Under this simple treatment the symptoms declined, and on the succeeding night she felt quite relieved; the lochia and secretion of milk becoming abundant, and the pulse soft at 125. After the lapse of some days she was up and well.

Mrs. M—— a married woman, aged 26 years, had very severe flooding in her lying-in. On the fifth day after, she was seized with pains in the abdomen, attended by excessive soreness to the touch, and fever. She got castor oil on the sixth day, which operated twice without giving much relief; in the evening she had a slight rigor and felt very weak; the milk had left her from the first moment of the attack, and the lochia

had nearly ceased. I saw her for the first time, on the night of the seventh day from her delivery, and found her lying on her back, with her knees up, complaining much, and incapable of the least motion on turning to one side or the other without the greatest torture. The pain was constant, but was increased to an excessive degree at intervals; coughing was very distressing to her, and the least pressure on any part of the abdomen made her scream; slow, steady, cautious pressure with the flat of the hand was unbearable, but she could stretch down her limbs without increasing her sufferings; her countenance, which was pale from the flooding, had an expression of distress, and the brows were knit as with pain; the skin was warm, the tongue moist and white, and the pulse weak, at 112; there was excessive tenderness of all the lumbar vertebræ. The abdomen was fomented, and she got two grains of calomel and a grain of opium every third hour. After the second dose the pain abated, and she got a good deal of sleep, but in the morning the symptoms returned, and the pills were resumed with the same good effect as before. The tenderness of abdomen was now rather less, though the bowels appeared more full, not having been moved for the last forty-eight hours; she could turn from side to side with less suffering; the pulse still 112, and the skin warm and moist; she took castor oil, and afterwards pills of aloes and extract of henbane every second hour, which operated freely; she passed a tolerable night and seemed easier, but got the pain more violently than ever on the following morning, when it was relieved by 40 drops of laudanum; it continued in the intervals of these paroxysms, but not severely. In the evening the paroxysm recurred, and was again relieved by the laudanum. Notwithstanding these recurrences she felt herself on the whole much better, could turn from side to side with more freedom, and wished for nourishment; she complained of some headach. She was now directed to take eight grains of sulphate of quinine three times a day, and to repeat the laudanum if seized with the pain; I also allowed her to get a little chicken broth

or beef tea. She passed a good night, and although the pain recurred at intervals on the following morning, it was much less severe; she threw up some aloetic and henbane pills which she had taken to free the bowels: the abdomen, though still painful on pressure, was much less tender; the soreness of the spine was nearly gone, and her milk was returning. As her bowels had not been moved for the last thirty-six hours, she got some castor oil, and the quinine and broth were continued; she complained much of headach.

The pain recurred again violently in the night, soon after which the flooding returned for a short time, and then ceased; the pain was relieved by a grain of opium; felt much better through the day, and ate a little meat without permission; the soreness of abdomen almost gone, and none whatsoever of the spine; her headach was better; she continued her quinine and nourishment, and was well in a few days.*

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* The above case at the first moment I was called to it, so perfectly answered the descriptions given of simple peritonitis, (see Abercrombie on the Viscera, p. 151,) and so truly resembled the cases of that disease which had fallen within my own experience, that I watched its progress with much anxiety. I was indeed somewhat distrustful of the diagnosis, for although the extreme tenderness of spine would fully account for the tenderness of abdomen, the great torture experienced on the least motion from side to side, was a symptom more characteristic of true peritonitis than of a neuralgic affection; the progress of the complaint, however, fully satisfied me of its nature. I believe there is no practical physician will assert that a case of acute peritonitis would have been arrested by the treatment adopted, which was in fact little else than palliative. It was only on the first day of my attendance that the opium, combined with calomel, was given with any regularity, five grains having been taken within twenty-four hours; for although the pain recurred with equal violence, subsequently she had seldom occasion to take an opiate more than once or twice in the day. I may however point out one symptom in which the case differed from peritonitis—the ease and freedom with which she could stretch down her limbs. In spinal affections the least motion of the spine, especially turning or twisting motion, will sometimes increase the abdominal pain, but it scarcely ever occasions difficulty or uneasiness in the motions of the lower limbs.

I might multiply cases of this nature to an extent that would be tiresome, without making the point much clearer. Perhaps it may be said, that an experienced eye would have detected some anomalous symptoms in all of these, which would have led to doubts of their inflammatory nature. I am not disposed to deny that much may be inferred in such attacks from the suddenness with which violent symptoms supervene, the absence of deep distress and anxiety of countenance, and above all from a freedom in the movements of the lower extremities, unusual in acute abdominal inflammations. There was indeed at least one of these discrepancies observable in a greater or lesser degree in each of the cases detailed; but how are young practitioners to form a diagnosis on such grounds. People suffer similar degrees and kinds of pain with very dissimilar degrees of fortitude, and at all events any reasoning on such signs must be founded on comparisons, which, to be worth any thing, would imply an experience no young practitioner can be supposed to possess.

In all acute inflammations of vital organs, I believe that no spinal tenderness will be found, except where it existed previous to the supervention of the attack,* or where the spinal cord itself happens to be the seat of such inflammation. In all neuralgic affections, on the contrary, tenderness of some portion of the spinal column, *usually that corresponding to the affected organ*, may be detected, except in some rare cases in which it seems probable the ganglionic nerves alone are concerned. As these cases must still present a difficulty in their diagnosis, we must rest contented with those general characteristics, which, however vague or liable to lead us into error, are all we have to guide us, and all we have hitherto had to determine our opinion in that large class of neuralgic affections, for

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* Some writers on spinal irritation state that they have found spinal tenderness with inflammation of liver.

the detection of which I have here been offering a new, and I believe less doubtful sign.

The observations offered respecting the diagnosis of cases resembling acute inflammations, apply with almost equal truth to those resembling chronic diseases; I mean those pains affecting the chest, and attended by cough and perhaps oppression, leading to apprehensions of phthisis, in the side between the false ribs and ilium, suggesting affections of the liver or bowels, or the pubic region simulating disease of the womb or ovaries. It is, however, wholly unnecessary to extend this paper to an unreasonable length on the subject of those nervous affections, the nature and diagnosis of which I have discussed so fully in another place; I need only remark, that deducing inferences from acute or chronic pain, apparently affecting any viscus, without examining the state of the spinal cord, seems to me little less absurd than omitting to examine the state of the hip joint in those painful affections of the knee, the nature of which is not immediately obvious. In either case the disease may not exist at all in the part apparently affected, and the real source of the pain can only be ascertained with certainty by an examination of the origin of the nerves distributed to it, or of any organ with which the part affected may be supposed to sympathize.

Art. VI.—*Medical Problems.* By D. Griffin, M. D.

How are we to distinguish symptomatic from idiopathic intermittent?

In the month of March some years since, when intermittent fever was very prevalent in the country, James M'Namara, a healthy young man, twenty-one years of age, was attacked with fits of shivering, accompanied by pain in the left side of his head; these fits were succeeded by heat of skin and quickness of pulse of some hours' continuance, and ended in profuse perspirations. At first the paroxysms came on rather irregularly,

but afterwards they occurred on every second day, for the most part at the same hour, and with increasing severity. When I was called to see him, he had been suffering in this way about eight days; the paroxysm had just then gone off, and left him in a low, weak state, with slow, feeble pulse, and some headach, which was worst at the left side, but was no where very severe; tongue whitish, face pale, and covered with a clammy sweat; the bowels had been tolerably free all through, and he had scarce any thirst except during the fit. I ordered him a drachm of the compound powder of jalap immediately, and a tea-spoon full of Peruvian bark three times a day. On the 25th, the fits of shivering were repeated once or twice, but the bark being continued, they went off and did not return for some days. On their recurrence I was again called to see him; the fits of shivering were much more irregular than before; for the last four or five days he had pain in the vertex, and that which he had in the left side of the head was very severe about the left mastoid process and ear; he could get no sleep, and suffered much during the last two or three nights, but there was no tendency to delirium; his pulse was ninety-five and hard, his tongue whitish, and there was very little thirst; his face looked pale, was covered with a clammy sweat, and had an expression of great anxiety. There was a tumour over the mastoid process at the left side, which was soft, puffy, and fluctuating, on opening which, a quantity of extremely fœtid, brownish pus sprang out with great violence and much relief; the bone was carious to the extent of about a shilling.

As this patient lived at a considerable distance from me, I found it impossible to see him every day, or indeed at all as often as might be wished. After the opening of the tumour, he got a purgative, and had relief from the paroxysms for a day or two, but they came on again with more than usual severity, and occurred daily instead of every alternate day; they were also much more regular than before, the cold fit commencing exactly at noon, lasting about half an hour, preceded by a hot

stage of somewhat longer duration, and terminating in profuse perspiration. I saw him in one of the intermissions; the pain in the head was trifling, and there was no thirst nor heat of skin; the face was pale, the tongue slightly white, the bowels free, and the pulse calm, at about seventy-five.

As these paroxysms bore a perfect resemblance to regular intermittent fever, of which there had been for some months a good deal in the country, as the intermissions were so perfect, the pain so trifling, and above all, as it had before yielded to the influence of bark, I was not at all convinced that the symptoms depended upon suppuration within the head. I had never seen the shiverings attendant on internal suppuration so perfectly periodical, and there was no improbability in the coexistence of such an abscess as I described, with idiopathic ague. I determined, therefore, on trying the bark again: the subsequent history of the case proved, that though it once more gave very unaccountable relief, it was a remedy entirely inapplicable, and convinced me of the great necessity there is for minute inquiry in cases so very insidious as these are.

The discharge from the wound, which had been a good deal for some days, began to diminish considerably; the fits also disappeared, the intermission lasting from the third to the sixth of April. He was then attacked with pain in the head, very severe in the vertex, and mastoid process, and attended with violent shivering fits frequently in the day; great thirst, heat of skin, vomiting and delirium, no drink remained on his stomach; his face was flushed, his pulse hard, at 110, and he was very restless; the pupils of the eyes were not dilated, but he lost his sight in the night for about ten minutes; the case was at this period evidently hopeless, unless something further was done. I had brought no instruments for trephining with me, and the place was many miles from my residence, but at all events the necessity for it did not seem very apparent, as the matter was flowing with sufficient freedom from the opening near the mastoid process. The only other obvious indication was to di-

minish the inflammatory action, and with this view I took twenty-six or twenty-eight ounces of blood from the arm; the vomiting ceased soon afterwards, and he seemed somewhat relieved. It was then late at night, and on my return next morning, I found he had expired about 3 o'clock. There was no examination of the body; an application for that purpose is seldom successful with people of this class in the country, and was not in this instance made, as I knew it would be useless.

Some time afterwards a boy, 10 years of age, was brought to me by his mother. He appeared very ill, and the resemblance the attack bore to that just related was very remarkable. She said he was seized at first with pain in the head, accompanied by frequent irregular fits of shivering. In about a week from their commencement these assumed a more regular character, occurring precisely every second day, and followed by a hot and sweating stage, as in the case of intermittent fever. He had been affected in this way about a fortnight; when I saw him his pulse was 95, and weak, his tongue whitish, his face pale, anxious, and covered with a cold perspiration; the skin was cool, but he had much thirst and was very feeble; his bowels were rather confined, and he had lost his appetite; again looking on it as a case of ague, I ordered him half a drachm of pulv. jalap. comp. and some pills of sulphate of quinine three times a day.

The mother returned to me in four or five days to say that he was much better, the fits had gone off and his strength was improving; the thirst and debility were much less, and he was altogether so much better, that she thought he only wanted something to restore his strength. A repetition of the medicine was accordingly ordered. In about a week from this time, she came again to inform me that he had got a return of the shivering fits, that they now came on every day, sometimes twice or three times; that he had been growing gradually worse since their recurrence, and was at last very bad; she said that

he had a great oppression, with delirium, much restlessness, and excessive thirst; that he had been delirious the whole of the previous evening and all night, and that he had refused drink since morning. The oppression was hourly increasing, but he was still able to speak when she left him; she also mentioned that he had complained much more than usual of his head the last four or five days, and that there was a tumour behind his left ear; she now also for the first time told me that he had been long subject to an occasional suppuration in his left ear. I went home with her to see him, but he had just expired as I entered the house. On looking at the body it appeared to be much wasted, the tumour was situated exactly above the mastoid process, as in the other case, and was soft and fluctuating. I was anxious to examine the head, but knew it would be quite useless to make application for leave to do so. There cannot, however, I think, be a doubt that there was matter under the skull, and that it had made its way through, as in the former case.

I shall give one instance more of this formidable affection, which though not attended by ague, nor by any external appearance of abscess, proved on a *post mortem* examination to have been exactly of the same nature.

Mary Cronon, a married woman, aged 24 years, applied at the Dispensary, complaining of a pain in her ear. The case was looked upon as one of common cold, and did not attract any particular attention at the time. The sunk and distressed expression of her countenance alone a little surprised me; but I attributed it to her having been deprived of sleep for two or three nights. Some days after her husband called on me to say, that she had no relief from the violent pain, but was much worse, and unable to leave her bed. On calling at the house I found her in a most deplorable state. The pain had extended from the ear to the back of the head; the countenance was heavy and stupid; the pupils greatly dilated; the skin warm, and covered with clammy perspirations; the tongue white; the

pulse small and very feeble, at 110, and there were constant retching and distressing thirst. On pressing the right hypochondrium she complained of pain. I had now no doubt of the nature of the case, but it appeared to have passed the period at which bleeding could be of any avail. The head was shaved; cold lotions were applied to the forepart, and the whole occiput was blistered; active purgatives were also ordered. She grew more listless and heavy on the following day, and was in some degree deaf, but was sensible, and when spoken to in a loud voice answered questions distinctly; the skin was cold and covered with clammy moisture; she complained of the intensity of the pain in the head, and, for the first time, in the back of the neck; she took drink well, but the retching continued at intervals. In the evening she roused herself up, apparently excited by a strong presentiment of her approaching dissolution; called her friends about her; said she felt she could never recover, and in a very affecting manner recommended her little child to the care of her mother and husband. As if she had then accomplished all that she could have any concern for in life, she sunk back into the same listless state as before, equally indifferent to the cries of her infant or the complaints of her family; she moved on the pillow with caution, complaining dreadfully of the back of the neck, and now and then sawing the air to and fro with her hands; she suffered intensely whenever her head was raised to give her drink, and at night had involuntary evacuations. I saw her, for the last time alive, on the following morning; she was lying on her side, frothing at the mouth, with a cold, clammy skin, ghastly expression of countenance, the lips pallid, and the eyes filmy; her pulse still distinct, at 95; she spoke rationally, and complained of the pain in the neck when questioned. Anxious to ascertain whether she could swallow, I had her raised to give her some drink, to which, however, she strenuously objected, exclaiming, "'twill choke me, 'twill choke me!" she was, however, induced to make the attempt, and had such difficulty that it was near suf-

focating her; her head was laid back on the pillow, and she expired in about half an hour.

The examination was made three days after death. There was unusual difficulty in detaching the cranium from the close adhesion of the dura mater; the pia mater was not particularly vascular; there were about two ounces and a half of water in the lateral ventricles; the choroid plexus and velum interpositum were unusually pale; the pineal gland was very soft, and contained none of those gritty particles commonly met with. On raising the tentorium, the cerebellum was found covered by a layer of thick pus, which extended over the upper surface as far forward as the cavernous sinus, into which it had passed through the aperture in the dura mater which transmits the fifth pair of nerves; it had also insinuated itself into the meatus auditorius internus. The pia mater and arachnoid membrane investing the cerebellum were highly vascular, and spots of extravasated blood were observable; this was also the case at the base of the cerebellum and the commencement of the medulla oblongata, though no matter was found there. The quantity of matter on the whole amounted to something more than an ounce. The substance of the brain and cerebellum differed little from the natural state; no other morbid appearances were found except in the liver, which was considerably puffed out below the ribs, with a round thickened margin; it was of a bluish colour, and its substance was of a soft spongy texture. She had never complained of her side until after she was attacked with pain in the ear.

The cases of this description recorded in medical writings are very numerous, and many of them have been found to resemble intermittent fever so strongly, that their real nature was entirely unsuspected until the progress of the disorder disclosed it, though generally at a period too late for relief; it is, therefore, a matter of the utmost consequence to be able to distinguish them in the early stage of their attack, the only time we can hope to find our efforts available. Unfortunately, however,

in diseases such as these, which are not always very rapid in their progress nor constantly acute in their course, it seldom happens that we are consulted early enough to have it in our power to prevent the occurrence of suppuration, and as the only rational practice, if the symptoms indicate that it has occurred, would be to trephine as soon as we can ascertain the seat of the matter, it would be a vast advantage to be able to tell when this has happened. We are much in want of facts on this subject, and it would require considerable experience in cases of this kind, *to be able to determine what connexion the rigors, or intermitting paroxysms, have with the actual existence of suppuration.* In the present state of our knowledge, however, their occurrence is the first circumstance that usually leads us to suspect that it has occurred, but we see that in their close imitation of a much less serious malady, they are, in some instances, likely to deceive us. The pain in the head in itself, except when fixed to one spot, cannot give any particular indication, as in common colds, in disordered stomach, and in the paroxysms of all intermittents, it is very severe. It is probable that more may be gathered from observing the changes of countenance closely, than from any other symptom whatsoever. It will, I believe, almost always be found, that even where the intermission is complete in other respects, the countenance bears evident signs that the patient is labouring under a serious and dangerous disease. In the advance of the disorder it becomes still more indicative of the deadly mischief which is going on within; the face and forehead are covered with a cold and clammy sweat; the features are sunk and ghastly, and there is on them an expression of extreme distress and suffering. These signs are not altogether peculiar to the cases before us, but, I think, may be observed in all instances, whether chronic or acute, in which matter is forming beneath the skull. I should, perhaps, except some of those, in which this state of things has arisen from external injury, as I think I have seen some in which this expression was not strongly marked, or was altogether wanting.

It seems probable, that in the instances given, the inflammation was neither violent nor extensive in its first attack, but that it was confined to a small spot which suppurated after the inflammation had lasted some days, and, by pouring out matter, separated the dura mater in its immediate neighbourhood; this in its turn becoming inflamed, added to the quantity of confined matter and to the severity of the symptoms, and, at length, from the continuance of this process, and the impossibility of the matter finding an outlet, the inflammation became very extensive and intense, adding every hour to the urgency of the symptoms and the certainty of the event. This view of the circumstances seems to be supported by the fact well known to medical men, that diseases so serious and dangerous as these, very often have their origin in the simple and apparently trifling fact of a child having a purulent ear, and leads us to perceive, in the strongest manner, the great difference there is with regard to the event, between the brain, when any part of it or its membranes is in a state of suppuration, and other parts of the body. The bony covering which is placed round it, to protect it from injury, is, when suppuration occurs in the brain, the main cause of destruction. In any other organ of the body, even in many of those which are concerned in the most important vital processes, large abcesses frequently are formed, and many of the persons affected with them escape, and even enjoy perfect health afterwards; but in the brain the smallest abscess occurring, even so small a one as sometimes in slight cases of whitlow separates the nail from the finger, is obviously attended with the utmost danger to life, and must necessary prove fatal except in the small number of those cases which come within the reach of surgical aid, and the still smaller number in which the matter is absorbed.

Hence it should be a constant rule of practice in the treatment of diseases of the brain, *never to admit even a hazard of inflammation, however slight or circumscribed, terminating by*

suppuration. In other organs, if it occur, there is still some hope left, in the brain there is virtually none.

It seems evident, that nothing but a very clear understanding of the real nature of the affection, and that at an early period of the illness, would have saved the patient in the first of these cases. It may perhaps appear that at the time the abscess was first opened, the trephine ought to have been used, and a more free opening given to the matter; but independent of objections on anatomical grounds, arising from a consideration of the parts among which the suppurating portion of the brain was situated, it must be remembered that the bone had become carious. The matter had worked its way through it, and on opening the abscess had flowed out freely with all the relief that could be expected from any operation.

In the third case, if the woman had been largely bled on the first moment of her applying at the dispensary, and other active measures adopted, the suppuration of the parts would perhaps have been prevented. When I saw her on her bed, and ascertained the real nature of the attack a few days afterwards, I believe that to a certain extent it had already taken place, and that all remedies were useless. My brother mentioned to me that he visited a boy similarly affected. He found him in a chair, hanging over the fire, with his head resting on his hand; he had a dull, depressed look, the iris was sluggish in its movements, and somewhat dilated, and he complained of dreadful headach, which had commenced by pain in the ear a few days before. He was immediately bled and leeched, got saline purgatives, with tartarized antimony, and had his head shaved and blistered. He nevertheless went on from bad to worse, and died in eight or ten days, like the others, comatose and paralytic.

With regard to the diagnosis, as it refers to intermittents, more may generally be gained by taking notice, whether the symptoms correspond in every particular with the apparent disease, than by other means. Thus, for instance, if it is ob-

served that the paroxysms are in any degree irregular, it will lead to suspicion, because the irregular and complicated intermittents are not so numerous as those in which the paroxysms return at regular periods. If there be, besides this, a fixed pain in any part of the head, with restlessness, want of sleep, complete loss of appetite, and constant whiteness of tongue, it becomes still more probable, however perfect the remissions, that the symptoms are dependent on disease within the head, and if with these circumstances there is observed such a state of countenance as we have described, the nature of the case can no longer admit of question.

There were two very remarkable circumstances in these cases; one of them I have partially alluded to, and the other I have until now omitted to notice. The first was the close resemblance the paroxysms which occurred in them bore to those of regular intermittent, and the second, their having been relieved by the bark and sulphate of quinine. With regard to the former of these circumstances, it is necessary to mention, that the paroxysms came on for some time in both of the cases with considerable regularity. There was also a great likeness in the circumstances of the paroxysm; each fit was not merely an attack of shivering or rigor, such as occurs commonly as a symptom of the formation of matter in internal parts, and which usually goes off after a few minutes; each attack was begun by a long continued and severe rigor, which was followed by a regular hot stage that lasted some time, and ended in profuse perspiration, bringing with it for the time all the relief that would have occurred in regular intermittent; and this is another circumstance in which the resemblance to these diseases was extremely striking. The intermission was usually as perfect as it would have been in any one of them. The face was pale, the pulse calm, the thirst gone. The patient complained but little, and the only point on which they seemed to differ was that the patients could get but little sleep, and that when once

the rigors had completely set in, the appetite never returned in the intervals.

It appears difficult to account for this great resemblance between two affections that are in other respects so dissimilar, but the difficulty will not perhaps continue if we pay attention to the following circumstances:

When the symptoms of diseases, which are very different in their nature, bear any resemblance to each other, it sometimes happens, from the great difference there is in the diseases which produce them, that the symptoms themselves, as supposed to be also in their nature dissimilar, we are apt to associate them with the causes from which we think they arise, and to imagine a difference in the former, because there is one in the latter. This association, however, is very mischievous in medical practice, and we should ever be watchful not to be misled by it. In a science surrounded with so much obscurity as that of medicine, it ought to be our constant effort to form habits of analyzing the process by which we have arrived at the opinions we find ourselves to hold, to observe whether they are founded in reason or probability, and to be ever on our guard against that, not unnatural, but most delusive principle of assimilation, which makes us jump at conclusions almost without our consciousness, and which, in circumstances like these, especially has a strong tendency to lead us astray. We ought, in short, always take care to distinguish the symptoms which are the facts of the case from the causes of them, which are almost always matter of opinion that may be well or ill founded, and remember that the causes being unlike does not necessarily make the symptoms different. The contrary conclusion, if adopted, would tend to puzzle us in the commonest cases, and would make the consideration of all obscure ones exceedingly difficult; but more than all this, it is not borne out by facts. For independent of the proofs of this that might be drawn from a consideration of other affections, intermittent fever itself is seen in connexion with a variety of causes, and though the influence of marsh affluvia is

perhaps the most frequent one, it often occurs in situations where the existence of this agent cannot even be suspected: they are said to be produced by sympathy and by contagion. "Two children," says Mr. John Hunter, ,' had an ague from worms, which was not in the least relieved by the bark, but by destroying the worms they were cured." "We have in like manner," he says, "agues from many diseases of particular parts, more especially from the liver and spleen, and from an induration of the mesenteric glands." One of the continental writers gives an instance of an intermittent produced by repelled herpes, and another of one which was produced by suppressed lochia.

But these paroxysms are capable of being produced by other causes also. The application of caustic to strictures in the urethra, it is well known, has frequently brought them on, and in some cases even with the utmost severity. I remember Mr. Kirby's speaking of a case in which he made use of caustic, in which its application was followed by paroxysms so severe and intractable, that the patient lost his life by it. Mr. Kirby was never an advocate for the use of caustic in such cases afterwards, which, though from motives different from his, seems to have been since almost entirely abandoned.

Now since no difference can be shewn in any thing but the cause of these paroxysms, it is perhaps too much to assume that they are of a different nature in all these distinct cases. Indeed when we consider the number of agents which are capable of producing them, it is obvious that the immediate cause of these paroxysms lies not in any particular organic lesion, but has its origin in some disturbance of the nervous system, and in the nature of the laws which govern it both in health and disease. It seems probable, too, that the action of the (so called) remote causes, consists in producing the peculiar state of that system which is always present with these proxysms, and the existence of which is indicated by them; and that the paroxysms themselves have no more connexion with marsh miasmata, than they

have with any of the other causes which occasionally give rise to them, or than any one symptom of any disease has with the remote cause which produced such disease. This conjecture as to the identity of the state which exists in diseases so very different in connexion with these paroxysms, seems to be confirmed by the fact of their having been benefited by the bark. The reason why the effect of the bark could not be permanent is evident.

We frequently see it mentioned in medical writings, that the shiverings from internal suppuration have often been mistaken for those of intermittent fever, and treated accordingly; but surely it would have been more practical, as they are sometimes capable of imitating them so perfectly, to point out to us some means of making the distinction.

The suppression of the paroxysms in our two first cases by quinine, while the disease which occasioned them went on to a termination, uninfluenced by the remedy, shews that the paroxysm was merely a symptom, and not even an essential symptom of the disease; it is of consequence then, (and this conclusion may be regarded as of general application,) in drawing inferences from the subdual of symptoms by any remedy, to consider whether such symptoms are essential or not, whether they are sympathetic affections which might exist without the local disease, or with a very dissimilar one, or whether they are necessary and inevitable attendants on it; which arise, and live and die with it. I remember a case in which a long-continued and obstinate pain in the head, after having been relieved by solution of arsenic, was followed by hemiplegia; another was related to me in which, after violent hectic with harassing cough was allayed by quinine, an extensive tubercular cavity appeared in the lungs after death; and a third in which a similar cough, after being perfectly appeased by quinine, henbane, and cicuta, was succeeded by hæmoptysis.

The great mistake we are apt to make about intermittent fever, is in the disposition to regard it as an idiopathic disease

rather than as a symptom; yet as the former we meet with it very seldom except in ague districts, while as the latter it falls under our notice every day. It is met with in the form of hectic fever; it is met with from irritation in the urethra; it is met with in spinal irritation; in hysteria; and as an attendant upon abscesses in the viscera. *It is in fact symptomatic of various diseased conditions, and bears the same relation to idiopathic intermittent, that symptomatic inflammatory fever does to the idiopathic continued fever.* The former may arise from local inflammation, from disordered bowels, from worms, from nervous irritation, &c. The latter is almost always produced by marsh miasma or contagion.

In conclusion I may remark, that the diagnosis of diseased conditions of the system, indicated by paroxysms of intermittent fever, deserves more consideration than those who have not fallen upon such cases as I have related, are likely to bestow upon it. A close study of it, with all its doubts and difficulties, can alone prevent us from confounding a very treacherous and fatal disease with one that in these countries at least is seldom attended with danger. If the inexperienced practitioner gives himself the habit of regarding paroxysms of intermittent fever as a symptom sometimes indicating a state imminently hazardous to life, at other times by no means so, his first care will always be to observe what degree of evidence there is for the existence of the most dangerous state with which they are so often found connected. In this disposition of mind, he will seldom be inclined to overlook any means which the case may afford for obtaining an insight into its real nature, or to undervalue any testimony that may be present for the existence of the latter condition.*

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* It is singular how protracted some of those cases of intermittent fever dependent on internal suppuration are. My brother tells me he was lately to see a patient in consultation, who had been attacked with daily paroxysms of ague from a few days after her lying-in, which took place about three weeks before. The only

Art. VII.—*Observations on Diseases of the Nervous System.*

By Charles Lendrick, M. D., T.C. D., King's Professor of the Practice of Medicine in the School of Physic, Physician to Mercer's Hospital, &c.

[Read at the re-union of the College of Physicians, November 18th, 1835.]

Whatever opinion may be entertained of the doctrines of Gall and Spurzheim, it cannot be denied that their anatomical researches have led to the establishment of valuable pathological principles, to the correction of prejudicial errors, and to the solution of several difficulties connected with diseases of the nervous system. For a long time it was considered that there were two distinct origins of the nerves, the brain and the spinal mar-

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circumstances in which it could be at all said to differ from regular quotidian was, that the paroxysms sometimes, though rarely, occurred twice in the day. The patient had no apparent complaint with which the affection could be connected when my brother saw her; but from the sunk expression of countenance, the dull expression of eye, and excessive debility, he had little doubt that suppuration had taken place in some internal organ. There was no soreness of abdomen, no uneasiness in the hepatic region; there was no cough, the lungs sounded well, and the respiration was clear and natural when examined by the stethoscope. As he could not learn that there had been any symptoms of uterine or other local inflammation, and there appeared to be so much obscurity about the case, he recommended the continuance of the quinine, wine, and broth, which she had been already taking; believing, on the one hand, that there was a bare possibility of its being a case of idiopathic ague, and that at all events support was requisite, and on the other, that if the affection was symptomatic of suppuration in some internal viscus, it would under any treatment prove fatal. The quinine suppressed the paroxysm once or twice for a day only, when it recurred again, and after lingering for a week or ten days longer, the woman died. On examination, about four ounces of matter were found effused in the cellular substance, which had formed a sac round it, close behind the right ovarium, resting on the sacrum, the fallopian tubes at both that and the opposite side were full of pus, and there was about an ounce of it in the fundus of the uterus, which was all through lined wiih a black, gangrenous-looking slough. Here was a case of ague, undistinguishable from regular quotidian, continuing for a whole month, and yet occasioned by internal suppuration.

row; and although some phenomena could be explained on this hypothesis, there were others that militated against it. For instance, it was obvious that disease or injury of one side of the brain must, on account of the decussation of fibres, impair the sensation and movements of the opposite side of the body, and that similar affections of the spinal cord would produce paralysis of the parts beneath; but to these general principles, there were exceptions which probably first led anatomists into a more minute examination of the nervous system at large. For instance, it occasionally happens that diseases of one hemisphere of the brain is attended with paralysis of *the same* side of the body; sometimes the alteration of structure is found only in the spinal cord, the symptoms during life indicating (on the common hypothesis) disease of the brain, or *vice versa*. Paraplegia is observed to be connected with, not (as is usually the case) spinal but *cerebral* disease; or, after the pathologist has predicted from the symptoms during life, the site and form of the disorganization, he finds both brain and spinal cord sound.

In consequence of the labours of the distinguished anatomists I have mentioned, it has been established, that the nerves have not their *origin* either in the brain or spinal marrow, but in the minute nervous fibres usually diffused throughout the body, which were formerly considered as the termination of the nerves, but are in reality the commencement of the organic structure of the nervous system, and its link with the invisible principle of life.

It may be right to inform our *non-medical* visitors,† that the nerves of each side of the body form two fasciculi, included within the spinal marrow, and which inosculate near its upper

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* J. Meckel considers these minute fibres to be surrounded by *an atmosphere*, which he conceives to be the medium of sensation. This opinion of the most distinguished of our modern physiologists is nearly that held by many of the ancients.—*Anatomie Generale*, traduit par Jourdan et Breschet, Partie I. No. 173, Paris, 1825.

† Many such attended the re-unions of the College of Physicians.

extremity ; the fibres of the right side thus reaching the left hemisphere of the brain, and *vice versâ*. Their further distribution has not been agreed on among anatomists. They have been traced to and even into the cineritious substance, and many are reflected through the commissures, so that several of the fibres of the *right* side of the body, after passing through the left hemisphere of the brain, finally reach the *right* hemisphere, and similarly with respect to the nerves of the left side. We are not authorized in concluding that the nerves terminate any more than that they originate in the brain. A remarkable connexion, however, is here established with the senses of seeing, hearing, smelling, and tasting, and new sympathies are formed, some of which are only developed during *disease*.

It will much facilitate our comprehension of disease in general, to bear in mind the *diffusion* of the vital principle, and not to assign to the nervous expansion termed *the brain*, more than its due share in its production. Strict observation will indeed induce us to come to the conclusion, that what are called *cerebral* diseases, often have their site in other parts of the nervous system, the brain being *secondarily* involved, and even occasionally free from disease ; at the same time, from the province assigned to it in the animal economy, any injury or disease having its site here, will be *more** readily and universally followed by effects involving the system at large, a fact so universally conceded as not to require further advocacy.

I believe, however, it will frequently be found that paralysis, convulsions, and other symptoms connected with disorganization of the brain, are joint effects of the same cause ; that is, that the symptoms and the disorganization are *both* the results of the disease or injury, and that the former are *not* the effect of the mere cerebral *lesion*. Many such lesions are found where symptoms, usually supposed to correspond with

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* Irritation of a nerve, teething, worms, &c. will cause epilepsy and other diseases generally referred to the brain.

them were not observable during life; for instance, considerable extravasation of blood after injury, or of serum during disease, and yet no paralytic symptom. I have also seen the skull fractured with a spade, and mud, bones, and dissolved brain mingled together, and yet the patient recovered without what are termed cerebral symptoms taking place at all; on the other hand every one has seen such symptoms in their most aggravated form, without any cerebral lesion whatever.

From before the time of Morgagni every effort that human ingenuity could exert has been displayed, in endeavouring to build disease upon the basis of diseased *structure*, so much so, that at the present day a case without "*the dissection*" is looked upon as of no value, and yet it is clear that this is not *the road*. Doubtless, from the intimate connexion between the corporeal and the spiritual, any considerable deviation from ordinary structure must produce functional disease, and aggravate it when already in existence; but in the majority of instances, this deviation will be found to be more an effect than a cause, and by no means an invariable one.

The disproportion between symptoms during life, and the amount of disorganization, whether in the brain or other important parts, has for some time engaged the attention of pathologists. These difficulties may, however, be in some degree surmounted, by considering the nervous system as *a whole*, and by bearing in mind the disproportion between cause and effect in *its* diseases, as dependant on *idiosyncracy*. In this respect Mr. Travers has conferred an inestimable benefit upon medical science, by directing its votaries to the study of spiritual rather than material causation. In one patient the slightest injury will be followed by derangement of the functions and death, while a great amount of it will in another scarcely produce a morbid symptom. Can we then wonder that these anomalies thus observed in a corner of the nervous system, and in a part (such perhaps as a limb) not extensively affecting its functions, should also be displayed, when the *centre* of this

system is itself the site of injury or disorganization; and that we should here, as in less important parts, have, according to the slight or considerable degree of the constitutional irritability of the patient, much disorganization of the brain, and few symptoms—or, severe symptoms, and little appreciable disorganization? Hydrocephalus and insanity, diseases usually referred exclusively to the brain, illustrate both the principle of extraneous origin, and also the importance of its occasional adoption in practice. In many cases which ultimately prove hydrocephalic, the disease in its early stage seems to affect the functions of the stomach and bowels almost exclusively, and on *post mortem* examination, we find the morbid appearances in the alimentary canal obviously of *earlier* date than those in the brain. With respect to the influence of this fact on treatment, we frequently observe, in families where the disease has already proved fatal, the early symptoms which formerly ushered it in displayed in other individuals, and the evil warded off by remedies acting on the *digestive* system alone, *where* the functional derangement and first change of structure have appeared in fatal cases, and where the remedies have been directed in those successfully treated must certainly our notice be directed in determining the "*fons et origo mali.*"

It is well known that the experienced Pinel referred insanity rather to the alimentary canal than the brain. In protracted cases we usually find a superficial hardening of this organ, but if we consider this as *the cause*, how are we to account for the disappearance of insanity, and the non-occurrence of this disorganization where the treatment has been what is called *moral*, or exclusively directed to the *mind?* That such is often the case the manager of every lunatic asylum is aware. Two of the most well marked *structural* diseases, softening of the brain and pulmonary consumption, are also in some cases distinctly referrible to a morbid state of mind, arising from grief, anxiety, &c., and the former has sometimes been cut short in its progress, by merely altering the patient's

mental occupation; as I knew to occur in a patient of the late Dr. Cheyne, in whom the peculiar symptoms of softening of the brain had appeared precisely similar to those which had ushered in that disease in a fatal form, in the case of his father, and two brothers.

That paralysis is often the consequence of mechanical injury of the brain, every one is aware, and, as has been already stated, the effects of injury inflicted *here* are more frequent, considerable, and extensive, than at any other part of the nervous apparatus, such effects having a marked tendency to proceed towards the remote ramifications of the nerves. This, however, does not impugn the occasional commencement of disease in *other* parts of the system. When this is the case it will sometimes be merely functional throughout; occasionally, however, it will *fix* and disorganize some part of the structure. This generally takes place in the brain, and involves that hemisphere (the opposite) where the fibres are most numerous; sometimes, however, it affects the fewer fibres of the *corresponding* hemisphere. Where both sides of the body are affected, the disease does not *generally* extend to the brain, but fixes in some part of the spinal marrow; although it *may* attack the union of the fibres in the brain itself. It is not therefore so surprising as it seems at first, that hemiplegia with disease of *the same* side of the brain, and *paraplegia* with disease of *the brain*, and not of the spinal marrow, should afford exceptions to the general rule, as to the relative position of the paralyzed parts, to those of the brain or spinal marrow which suffer under disease.

Every one knows that partial and progressive impairment of sensation, or motion, or both, may precede by a time varying from minutes to years, a confirmed apoplectic or paralytic attack. The former is generally considered as cotemporaneous with the development of diseased structure in the brain, but in my opinion without sufficient evidence. We know that similar affections are attendant on rheumatic and other inflammation of *the nerves* distributed to the affected parts, and in many cases

where patients have died before the full development of paralysis, but in whom it has existed partially, no cerebral or spinal lesion is discoverable. So that it appears probable that slow paralysis is a merely local disease at first, much more frequently than is supposed, although one always likely to involve the brain in the end. It would seem indeed, that although the effects of injury have a disposition to flow from the brain *to* remote parts, those of disease often assume a contrary direction, that it commences in remote parts, passes to the brain, and disorganizes during its course, but especially on its arrival there. What induces me to think that paralytic disease *often* (for I am contending for nothing more) proceeds from remote parts *to* the brain, and not in the contrary direction, is the circumstance that in slow cases where the progress is most easily observed, the affections of the limbs are usually first in order of time, and the date of the impairment of seeing, speaking, and other functions more immediately connected with the brain are subsequent, which is not the case, where the entire disease can be connected from the first with this organ, as where it is referrible to a tumour or injury of the brain itself. The action of *strychnine* is also illustrative. Its effects both on sensation and motion (such as prickling and twitchings) are progressive throughout the paralysed limbs, and precede the restoration of their power, but if we increase the dose the brain becomes affected, and injuriously so. This progress of the action of *the remedy* from remote parts towards the brain, establishes a probability of the same order *occasionally* existing with respect to *the disease*, and if so, the importance of not looking so exclusively to the brain (and nothing but the brain) in the early period of such cases, as if the affected parts, the nervous system at large the other functions, and the mind were of no importance at all.

Isolated primary organic disease is indeed a rarer affection than is generally supposed. In the majority of instances many functions are presented in a morbid state, and it by no means

follows, that the organ subservient to that which seemed most depraved before death, will subsequently present the most decided marks of disease. For instance, apparent disease of the brain will be followed by apparent disease of the heart, to which the fatal result will seem to be referrible, and yet the post-mortem appearances will perhaps be tubercles in the *lungs*, or some disease of the *liver* ALONE. This diffusion of the principle of vital destruction, and its final concentration in the form of an unexpected disorganization, is indeed far from rare.

The principle which I am urging, and which I frequently take pains to impress on my pupils, is, that if we would treat disease as disease *is*, and not what theorists wish it to be, we must generalize—consider the brain, the nervous system, the various organs and their functions, as well as *the mind* itself, as *a whole*—a grand system—in *any* part of which disease may take the lead—in any part terminate—and in any material part or parts disorganize.

One very prejudicial consequence of viewing disease as too dependent on variation of structure, is the practice of indifference as to treatment, and to the state of the mind after organic change is supposed to have gone a certain length. Under the idea that no harm can be done, because good is impracticable, it is not unusual to treat the patient with "*candour*" as it is called, and to explain fully to him his danger and its degree. The practitioner may, however, rest assured that this is the way to render certain and speedy, what was before remote and perhaps uncertain ;* and that if a fatal result be inevitable, it will be accelerated in proportion to the an-

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* I knew a person who had for years enjoyed tolerable health, but was affected by a slight cough, on the announcement (after a stethoscopic examination) that there was a cavity in the lungs, take to his bed, and die in a few weeks. The cavity *was* there, but it *might* have cicatrized, or at least its effects been delayed.

nouncement of its approach. It is surely unnecessary to state the reasons why life should be prolonged, or that it is the duty of the physician to do nothing that is likely to shorten it; in some cases, no doubt, where fancied security impedes the adoption of the requisite remedial measures, or renders the patient indifferent to the state of his spiritual or temporal concerns, it may be necessary to arouse him by some explanation of the seriousness of his case; but if it be not our object to hasten dissolution, the announcement of its hopelessness ought to be delayed as long as possible.

The following practical rules may, in my opinion, be deduced from the preceding observations:

1st. That almost *any* disease may end in disorganization of the brain, although the present symptoms may not present any of its characters. Hence *every* disease is deserving of notice, and ought to be *removed* as soon as practicable, notwithstanding the trifling name that may be ordinarily assigned to it.

2nd. That in treating those diseases which are usually supposed to be specially connected with the brain, their *other* connexions ought not to be forgotten. Hence every depraved function, whether of mind or body, ought to receive appropriate treatment, and our attention should not be confined to the brain itself.

3rd. That functional disease of the brain, and apparent disorganization there, sometimes terminate in real disorganization of some other part essential to life. Hence we ought to bear in mind the hereditary predisposition and previous diseases of the patient, and be in readiness to combat any translation of functional disease from the brain elsewhere, and which may in its latter situation seem slight, and sometimes be scarcely perceptible, although the disorganization may prove to be very great on dissection.

In the great majority of cases, the mind becomes affected where the brain is disorganized. Such indeed is the intimate connexion of the corporeal and spiritual essence *here*, that their

lesions generally correspond, whether disease originate in the one or the other. Thus, injury of the brain impairs the mental faculties, and long continued affections of the mind change the structure of the brain. We must also concede to phrenologists, that in many cases, at least the development of the mental faculties is *accompanied* by a peculiar arrangement of the parts of the brain; but in assigning the brain as the *cause* of these phenomena, we merely *assume* what we in vain endeavour to discover, and flounder out of our depth. Most (not all) hemiplegiacs show traces of disorganization in the brain, and the great majority labour under a greater or less degree* of mental imbecility, especially weakness of memory, particularly with reference to recent events. Sometimes all the faculties are blunted; on other occasions it would seem as if a corner were taken out of or left in the mind; there is general integrity with partial obliteration, or general obliteration with partial integrity. But whether all or some of the faculties suffer, in EITHER case the disorganization of the brain *may be* general, partial, or none. So that the corner is *not* taken out of the mind, *because* a corner is taken out of the brain.

I shall in conclusion relate a remarkable case of both the partial relation and obliteration of the memory in the case of a gentleman well known to some of our University visitors.

This gentleman had been highly distinguished for his scientific attainments. I saw him some years since, he was then slightly paralytic, and he did not seem to recollect me. I was lately informed of his death. His memory had become so completely obliterated for some time, that he recollected only two or three friends, and them but imperfectly. He had completely forgotten the use and nature of *letters*, and could neither

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* A distinguished Irish prelate, not long since deceased, preserved his faculties (which were of a high order) in their perfect vigour for years after he was completely disabled by hemiplegia.

write nor read a syllable, nor even comprehend the alphabet. He however perfectly recollected the use of *figures*, and could solve the most intricate *arithmetical* problems; but if *algebraical* symbols were introduced, (and he had been a distinguished algebraist,) the connexion with letters confused him, and he was completely at fault.

ART. VIII.—*On Amputation of the Penis.* By J. THOMSON, M.D. M.R.C.S.L., Surgeon to the Newry Dispensary, and Fever Hospital.

CONSIDERABLE diversity of opinion seeming to exist in reference to the best mode of performing the operation of amputation of the penis, this operation being necessarily performed frequently, and its issue, or at least the celerity of its cure, in a great degree depending on the manner in which it is executed; I am led to presume that the short statement of the two following cases, together with the few cursory observations thereon, may be not altogether unworthy offering to the profession.

CASE I.—John Crommy, aged 60, was admitted into the Newry Hospital, December 30th, 1835, with carcinomatous ulceration of the penis, which extended within an inch and a half of the pubic arch. The entire of the glans, and a considerable portion of the corpus spongiosum and corpora cavernosa were destroyed. The extension of the disease during the week previous to his admission, has been more than usually rapid.* The inguinal glands seemed in their natural state.

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* This, however, may be in some measure attributable to an application which he made use of a short time previous to his coming under my care, and which is held in high estimation by the vulgar in this neighbourhood, as a remedy in cancerous affections, under the cognomen of "Davis' Plaster," which gets its name from its supposed founder, the late Rev. Mr. Davis of Warrenpoint. The indiscriminate distribution of this plaster seems to me highly reprehensible; to my knowledge it has in many cases inflicted serious mishief.

Amputation being determined on, the operation was performed in the following manner, the 1st of January of the present year. The portion of the penis which was remaining was grapsed between the fingers and thumb of the left hand, gently forward (taking care not to extend the skin farther than the body of the organ, so as that a sufficiency of the integuments would be preserved to cover the wound,) and extirpated pretty close to the pubis, by one stroke of a small sized catlin. Three arteries were immediately and most easily secured, from the retraction of the corpora cavernosa and corpus spongiosum urethræ being much more rapid than that of these three vessels, and which consequently left them quite projecting. The wound was now sponged, and a gum elastic catheter attempted to be passed into the bladder. This, however, after a series of trials, could not be accomplished, owing most likely to some organic affection of the prostate gland, as the catheter passed thus far freely. The instrument was accordingly withdrawn, and having some of the best authorities for healing the wound without a tube being left in the urethra, I brought the integuments together at its upper and lower parts, taking every care to leave a free space between them for the passage of the urine, and covered the part with simple dressing, directions being given that when water was being passed, he should rest on his hands and knees so as to allow a direct exit to that fluid.

When visited the following day, he was found free from uneasiness, had a good night, passed water freely two or three times since the operation, and no hæmorrhage had taken place. Matters appearing to proceed so favourably, the dressing was now merely renewed, and all left as before. Nothing worthy of observation occurred during the three or four days subsequent to this, but on the sixth day there was experienced considerable difficulty in evacuating the urine, and this evidently depending on a contracted state of the orifice of the urethra. A bit of a small sized bougie was put into this passage, with

directions to have it replaced after each time of evacuating urine till again visited. When seen the ensuing day, it was found the bougie had slipped out during the night, that the urine now, even when strongly forced, could only be voided guttatim, and that a silver probe could with difficulty get admission into the urethra, from the almost complete destruction of its orifice. After in some measure widening the orifice with the probe, a small bougie was introduced; this being kept in a few hours a larger one was substituted, and this in its turn changed for one still larger, till by rapid dilatation a portion of the barrel of a large-sized writing quill was admitted; (a bit of its small extremity having been cut off, so as to form a canula, and preserve at that extremity, in order to protect the urethra, a somewhat rounded and as as it were compressed form.) This tube was now fixed in this situation, and the wound simply dressed as before.

From this period all matters proceeded favourably, the urine, when necessary, was freely passed through the canula, and the wound healed rapidly. The tube was retained in the urethra about a fortnight, at which time the orifice of that passage seeming sufficiently large and the entire of the wound healed, it was removed. A tendency, however, which the orifice had to contract, caused a necessity for one or two applications of nitrate of silver, when the cure appeared complete.

Case II.—A. T——, a farmer, aged 58, residing in the same neighbourhood as that from which the preceding patient came, and labouring under a similar affection, applied to me for advice in February last. A considerable part of the glans penis was then destroyed, the ulceration extended deeply about an inch and a half higher up on the body of the organ, and evinced the characteristics of true carcinoma. One of the inguinal glands was slightly enlarged. Immediate amputation was recommended, and performed exactly as in the former case. Four vessels were tied, a canula of about two inches in length

introduced into the urethra, and left projecting about half an inch out of its orifice, so as to keep the wound free from the contact of urine; the integuments, which were quite sufficient to cover the stump, were brought together by means of three sutures, (the ends of one of which were so fastened around the projecting extremity of the canula, as to retain this tube firmly in its place,) and a bit of simple dressing applied over all.

The success of this operation exceeded my most sanguine expectations. The pain after the wound was dressed was very trifling, the canula gave no uneasiness, the urine was passed freely through it when necessary, the ligatures were taken away, with the exception of the one which was used to retain the canula in its place on the sixth day; and the wound was completely healed on the tenth. No contraction of the urethral orifice occurred afterwards.

With regard to the mode even by which the mere cutting off of the penis is performed, surgeons seem very much unsettled. The following is Mr. Lawrence's method. "You cut circularly round the integuments of the penis, and when those have retracted as far as possible, you cut through the body of the penis: the integuments will then be loose enough to admit of their being brought over the stump of the penis." Sir A. Cooper, on the contrary, says, the preservation of integuments is not only unnecessary, but a great evil, by preventing a free escape of the urine. Sabatier tells us to draw the skin *towards* the glans penis, and cut through it together with the body of the penis by one stroke of the knife. And Mr. S. Cooper, in his excellent Surgical Dictionary, says, a circular incision should be made through the skin, and the corpora cavernosa and the urethra then cut through by one stroke of the knife, on a level with the cut edges of the integuments.

It will be seen that neither of these modes was exactly adopted in the foregoing cases, and that notwithstanding the amputations being performed at the greatest diameter of the

penis, particularly in the first, and without any precaution being taken to preserve integuments for covering the wound, there was, from the retraction of the corpora cavernosa and corpus spongiosum, fully a sufficiency for that purpose. Now if this be taken into consideration, together with the simplicity of the operations as above described, and the celerity with which they were performed, I think it should have a strong tendency to show that amputation of the penis, in all ordinary cases, should be performed by one stroke of the knife, and that consequently a preliminary circular incision, for the purpose of preserving integuments, is not only unnecessarily painful and tedious, but positively injurious, by providing a greater quantity of skin and cellular substance then would be necessary to cover the wound, and which would consequently lie over the stump in a loose sac, and prove a mere receptacle for matter, or if a canula were not kept in the urethra, a serious obstacle to the passing of urine. The body of the penis, when cut through in a healthy part, will, I am pretty confident, always retract in a sufficient degree to leave integuments for covering the wound; and if there be no healthy part, there should be no amputation.

Neither are surgeons by any means unanimous with respect to the propriety of bougies or catheters being left in the urethra after this operation. Mr. Lawrence entirely condemns their use. He says, "I have never found it necessary in amputation of the penis to do that which is recommended by most systematic writers, that is, to introduce a bougie or catheter, to keep the remaining portion of the urethra open. The truth is, that this proves an additional irritation; the patient makes water very well after the operation, and the introduction of such a substance creates an uneasiness that is not at all called for, so that you may dispense with the bougie with great safety."* Sir A. Cooper says, when the surface begins to granulate, a piece

* Lancet.

of bougie, two inches long, is to be worn constantly in the urethra to prevent its contraction, otherwise it gradually closes as the wound heals, and produces retention of urine.* The late Mr. Hey, in his "Practical Observations in Surgery," chap. 14, advocates the *occasional* introduction of a bougie; and the late Mr. Pearson, in his "Practical Observations on Cancer," says the introduction of canulæ are painful and altogether unnecessary.

It will be seen that on the sixth and seventh days after the operation, in the first of the above cases, the urine was avoided with the utmost difficulty, and merely guttatim; and this must have been owing to the contracted state in which the orifice of the urethra was found, as there was no redundancy of integuments, nor any other cause but the contraction discernible, which could in any way afford an obstacle to the free passing of the urine. I think, therefore, it should not only be inferred that contraction of the urethral orifice may take place after other amputations of the penis, as well as after the present one, if treated without bougies or canulæ, but that from the construction of the parts, there will even be a natural tendency to such, both from the retraction of the urethra and the extension of the granulations over its orifice.

With regard then to the means of preventing this contraction, which I conceive from the first should be strictly guarded against, as well to those by which the healing of the wound may be expected, I would give a decided preference to the introduction of a canula immediately after the amputation, and to the approximation of the lips of the wound by sutures, so as to endeavour to heal it as in other cases as quickly as possible. Until the canula was introduced in the first of these cases the urethra seemed to be gradually contracting, and the adjacent integuments and wound, from the contact of urine, beginning to assume an irritable appearance, but when the

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* Lectures by Tyrrell.

canula was used the urine was conveyed freely off, and the wound speedily regained a healthy aspect. And in the second case it will be seen how fully the canula answered the expectations assigned to it.

Those surgeons who declaim so strongly against the employment of bougies or canulæ, are likely in the habit of fixing these instruments in the urethra, with one extremity lying on the bladder, or of only introducing them occasionally. If this be the case, I have no doubt but they have generally been productive of all the inconveniences assigned to their use. But if a canula of about two inches in length be introduced into the urethra, immediately after the amputation, and properly retained in this situation till the wound be perfectly healed, I will affirm as confidently as the two preceding cases will allow me, that there will be very little, if any, uneasiness felt from its use, that the water will freely pass through it when necessary, (if it be sufficiently large to fill the urethra, which it should, in order to prevent the urine from escaping between it and the sides of this canal,) that it will tend very much to accelerate the healing of the wound, by its preserving it from the contact of urine, which, without the assistance of some such instrument, will be constantly producing irritation. And that its employment, speaking from the first of those cases, and from two other amputations which I saw performed where canulæ were not used, will *in general be necessary* towards the completion of the healing process, to prevent retention of urine from contraction of the urethral opening, which, I presume, at that period will, in at least the majority of cases, be found, without it or some similar assistance, very likely to occur. A catheter, when left in the bladder, is likely to induce considerable irritation about the neck of this viscus, as well as considerable uneasiness to the system at large; but neither of which, generally speaking, would be produced by the employment of a canula, of an inch or two in length, such as mentioned above. A quill canula of this length, (which seems preferable to either a silver or gum-elastic canula; the one being

altogether unresisting, the other not sufficiently so,) from the moisture and heat of the urethra rendering it somewhat soft and in a degree compressible, lies in this canal without producing almost any inconvenience, and appears to me vastly superior to a bougie of a similar length introduced occasionally, as advised by some surgeons, not only from its protecting the wound from the contact of urine, but from its admitting of our bringing the lips of the wound together, of retaining them in that situation without disturbance, and consequently of the chance of union by the first intention.

Being an advocate for the employment of a canula in the operation in question, I am consequently an advocate for the healing of the wound as quickly as possible. I consider the approximation of the integuments over its face of no small importance, if this can be effected without any risk in obstructing the urethral opening, as it with certainty can, when a canula is retained in this passage. By this means considerable pain and irritation may frequently be spared, and much time in the healing of the wound as frequently saved. It is said the corpora cavernosa do not regularly granulate nor unite to the skin by the first intention; but from the last of the above cases I would be inclined to doubt the entire accuracy of this statement, as I never saw the attempt to heal by the first intention fairly made after the amputation in question, previous to that case, and in that case it succeeded almost perfectly.

Though by no means an advocate for the unnecessary use of sutures, I would employ them in the present case in preference to adhesive straps; they retain the integuments together with certainty, which the latter in this situation, from the growth of hair, mobility of parts, &c., cannot accomplish.

Art. IX.—*Second Letter from* Doctor Lombard *to* Doctor Graves *on the Subject of Typhous Fever.*

Geneva, 18th July, 1836.

My dear Friend,

Let me add a few words to my former communication on your contagious fever of Ireland. After I had left Dublin I was very desirous to compare the cases of fever which I might meet, with those described in my former letter, and it is with that intention I visited the fever wards or hospitals of Liverpool, Manchester, Birmingham, and London.

In Liverpool the fever hospital is in a very healthy situation, built as it is in the highest part of the town, and surrounded by an extensive garden; the wards are not roomy, but the system of ventilation is quite adequate. The number of patients which were then in the hospital was not so considerable as in Dublin, but seemed to me to approach one hundred; the cases were most of them severe, and had exactly the same aspect as those which I had seen two days before in Ireland; the same typhoid appearances, the same papular extensive eruption, and in most cases the predominance of cephalic over all other symptoms. I saw in the Liverpool wards many patients who were older than fifty, and some who seemed to be at least seventy years old. The contagiousness of the Liverpool fever is quite clear from the number of persons of the same family who are attacked by it, and from the transmission to the nurses and medical attendants; I found only one nurse who hitherto had escaped it. The morbid appearances found on dissection bear the same character in Liverpool as those described in Dublin: viz. serous effusion in the brain, meningitis, pneumonia, and occasionally some injection of the mucous coats of the intestines: as for the ulcerations of the ileum and cæcum, they are occasionally, but by no means constantly, met with; their frequency varies with the different seasons.

In Manchester the fever hospital is by two-thirds smaller than the same institution in Liverpool, and it is by no means insufficient, as, when I saw it, one-half of the wards were empty; the total number of cases admitted in the year, was 392, of which forty-seven were cases of scarlet fever, and thirteen of small pox, so that the total number of fever patients in the whole year did not exceed *three hundred and thirty two;* a proportion far inferior to that observed in Glasgow and Liverpool, towns of very nearly the same population. The appearance of fever patients in the Manchester wards was exactly similar to that already described; that is, they had the same stupor, the same cephalic symptoms, and the same papular or typhoid eruption, which was at least as copious as in any case I had seen in Dublin or Glasgow. I saw some old people in the wards, and the same opinion respecting the contagiousness is entertained in Manchester as elsewhere. As for the morbid appearances, I was told the ulcerations were by no means always to be found in the intestines of those cases which had proved fatal.

In Birmingham there is no fever hopistal, but two fever wards in the general infirmary, and so limited is the number of typhoid cases, that there was not one in these wards when I visited the infirmary; the medical attendants told me that the fever cases were by no means frequent in Birmingham, and that in all those which had proved fatal, the ulcerations of the lower part of the ileum were always to be found.

In London the fever hospital is comparatively small, and contains few patients, and the annual number of admissions has been diminishing for the last three years; in 1835 it did not exceed 309, of which fifty-two were cases of scarlet fever, and seven improper cases; at the time I visited the hospital, there were not more than ten or twelve cases of continued fever, but those had the same appearance as the cases observed in other parts of great Britain; I remarked the same predominance of cephalic over the abdominal symptoms, and the same profuse typhoid eruption; diarrhœa is very seldom to be met with either before

or during the course of the disease. Serous effusion in the ventricles and other morbid appearances of the brain, are more frequently to be met with, than the abdominal complications which in the shape of ulceration are not to be found in more than one fourth of the cases, according to Dr. Tweedie's researches; and this proportion of ulcerations in the lower part of the ileum varies with different seasons, being much greater in autumn than at any other period of the year. The continued fever of London is very different now from what it was some years hence; it was then highly inflammatory, and local and general blood-letting could not then be neglected; while now the symptoms of depression and weakness have taken the lead, and so low is the present fever that wine, camphor, and spirit of Mindererus are to be given in almost every case.

Now, my dear friend, that I have stated the facts, let me presume to give an opinion upon what I have seen of your British continued fever. *I consider Ireland as the source of the continued typhous fever which prevails in Great Britain;* it is there that it is generated, or rather constantly transmitted from one individual to another; your fever is what the French pathologists have called *typhus, typhus contagieux, fièvre des armees, fièvre des prisons;* as it can be found from the delineations written in 1813, 14, and 15, when this fever prevailed wherever the armies met or sojourned. This fever has unfortunately its abode in Ireland, and more especially in Dublin, a fact which is made too evident by the records of one of your fever institutions, the Cork-street Fever Hospital, which received no less than 77866 patients in twenty-five years, and that hospital is not the only one where fever cases are admitted in Dublin.

Wherever the Irish journeymen go, they carry along with them the contagion of fever; the consequence is, that in Glasgow, where no less than 60,000 Irishmen are to be found, fever prevails to a great degree; in Liverpool the same fact is observed; in Manchester there is less fever, because the

Irish who live there are not so changeable a population as in Liverpool, being chiefly composed of settled families rather than of travelling journeymen. The same remark holds true for Birmingham, where typhoid fever is far less prevalent than in the above named places. In London, many days having elapsed between the departure of the Irish journeymen and their arrival in the metropolis, the chance of infection is consequently much diminished, and the consequence is, the great falling off in the number of fever cases, which might have been observed at a smaller distance. If London had as many cases of typhous fever as are daily seen in Dublin, the different London hospitals should contain at least three thousand cases, and the actual number is not the twentieth, perhaps not the thirtieth part of the above.

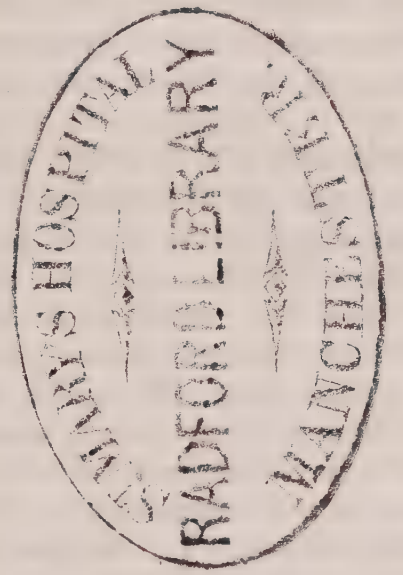
But the Irish contagious fever is not the only source of typhoid diseases in Great Britain; the sporadic continued fever observed in all parts of Europe is also to be found in the different towns of the British Empire. This fever, characterized by the follicular intestinal eruptions, and by consequent ulcerations, is to be seen in the different places above mentioned; in Glasgow it forms one-third of the total number of cases; in Dublin the proportion is much less; in London it is one-fourth, and varies in the different seasons, because the continued sporadic fever is much under the influence of the temperature, being more frequent in Autumn, than in Spring and Winter; a proof that the proportion of this sporadic fever is the cause of the greater proportion of ulceration cases found at times in the British hospitals, as already mentioned.

Having stated my opinion on your British continued fever, I resume it in the following theoretical view. You have two different fevers, one highly contagious, which I may call the *Irish Typhus,* and in which the cephalic symptoms predominate, to the exclusion of abdominal alterations; the other which is *sporadic,* and most likely not so infectious, and in which the abdominal symptoms are more predominant, so much

so that the follicular disease and consequent ulcerations are always to be found. These two different fevers are to be found in most parts of Great Britain, but the first is more prevalent in Ireland, and where the Irish come in great numbers; the other, similar to the European sporadic fever, is to be met with in all places, varies with the seasons, and is not necessarily produced under the influence of a contagious principle.

Allow me now, my dear Professor, to suggest to your medical friends, how interesting it would be for the medical world if researches were instituted, with the view of describing accurately your different forms of fever, in collecting a number of cases from the beginning of the disease to the post-mortem examinations. Such materials would undoubtedly throw much light on the pathology of fever, and might lead to some important practical result. And I know of no place where such researches might be so well conducted as in Dublin, both on account of the great supply of the subject of observation, and from the number of acute and scientific observers, which are to be met in your highly interesting medical schools.

H. C. Lombard.



BIBLIOGRAPHIC NOTICES.

Flora Hibernica, comprising the Flowering Plants, Ferns, Characeæ, Musci, Hepaticæ, Lichenes, and Algæ of Ireland, arranged according to the Natural System; with a Synopsis of the Genera, according to the Linnæan System. By James Townsend Mackay, M.R.I.A., Associate of the Linnæan Society, &c. &c.

We hail with pleasure the appearance of a "Flora Hibernica." We are pleased with the work before us on many accounts. It is edited by those Irishmen who are most probably the best calculated for the execution of the task. It may be objected that Mr. Mackay is not an Irishman. But surely he has sufficiently identified himself with Irish botany, to deserve being considered indigenous to the green isle. We are pleased with this work because it does credit to an Irish publisher. But more than all are we pleased, because it removes for ever the stigma of having neglected the natural productions of our native land. We perfectly agree with Mr. Mackay, that this censure was at any time one of too great severity. Whoever examines the works of Withering, Smith, or still later, the admirable Flora of Sir W. Jackson Hooker, will perceive how frequent and how valuable have been the contributions of Irish naturalists: the names of Turner, Stokes, Templeton, Drummond, Hutchins, &c. occur in every page. However, a complete and systematic collection of known Irish plants was a work much to be desired, and this we have presented to us in the present publication. It appears to us that works on practical botany may be divided into two classes. In the one the plants are arranged upon some easy and invariable plan, by means of which, we may with facility find out the name of any plant we happen to meet just as in a dictionary, we can, by recurring to the first letter of any word, readily discover its meaning. In the other class of botanical works, the plants are arranged

according to their resemblances and relations. In the same manner as the words of a language are distributed in its grammar. In the latter case every species in any particular group should accord with the rest in the greater number of its most important characters. But there is this necessary imperfection in works of the former class, that, as in a dictionary, a verb may immediately precede a noun, and an adjective, a preposition; so the plants must be associated, without any reference to their general organization; nay, we may almost venture to say, that a certain degree of discordance is desirable in an artificial scheme; inasmuch as the greater the difference between the associated species, the greater the facility with which they may be distinguished. In the work before us, Mr. Mackay has endeavoured to unite the advantages of both these arrangements, with what success we will presently attempt to determine. We feel that in taking up the critic's pen, when about to analyze the matured labours of so experienced a botanist as Mr. Mackay, we may with great appearance of justice be charged with temerity. Mr. Mackay will recollect, however, that his work is principally written for the benefit of the student, and that the latter is justified in examining, with becoming humility, what increased advantages of facility in acquiring information this work may afford him, and comparing its claims with other publications of established reputation; we consider it to be one of the most important privileges conferred upon the votaries of nature, their freedom from the petty jealousies and heart-burnings which distract other classes of scientific men. We are therefore convinced that Mr. Mackay will receive our remarks in the same spirit in which they are offered.

The "Flora Hibernica" is divided into three parts, the first comprehending the indigenous phænogamic vegetables; the ferns and characeæ, arranged according to the natural method, is especially the produce of Mr. Mackay's labours; the second part, containing the mosses, liverworts, and lichens, is by Dr. Taylor. The algæ, constituting the third part, are described by W. H. Harvey, Esq. We shall examine in turn the method and execution of these parts with such deference, as the very high characters of the authors demand.

Mr. Mackay defends the adoption of the natural method, "not only because it is the method followed by Dr. Allman, the learned Professor of Botany in Trinity College, and which although more difficult to beginners, is best calculated to give the student an accurate knowledge of the science." But to increase the facility of discovering the genus of a plant, he has superadded a synopsis of the genera, according to the Linnæan scheme. Now nothing could delight us more than the diffusion

of a knowledge of the natural arrangement of plants; we confess that we do not at all relish the sneers that are thrown out against it from certain quarters; we believe it to be founded on a grand physiological truth, that by means of it we have given to us a key capable of unlocking vast stores of knowledge. "On a dit depuis long temps qu' une science, est l'art de deviner ou de predire;" and we perfectly agree with Decandolle, that the natural system has so far elevated botany to the character of a science. We are afraid, however, that the supporters of this system often injure the cause of science, by injudiciously displaying the difficulties which are necessary to be overcome, rather than the facilities which it affords. Every botanist is aware that the natural system is founded on the comparative value of characters, and that these characters are of two kinds; the one differential, by means of which we are enabled to distinguish any especial family or genus from all others; the second descriptive or collective, detailing the features of resemblance existing between the members of the group. Now we acknowledge that we cannot see the utility of a descriptive character of a family in a Flora, no more than the perfect description of a genus or species; nay, we are inclined to think, a lengthened description rather injurious than otherwise in such a work; but if it be at all present, it should be most cautiously distinguished from the diagnostic or differential character. To give an example which may exemplify our meaning, we quote the characters of the Labiatæ, as announced in the Flora.

"Ord. 59 Labiatæ Infs. Labiate family.

"Calyx tubular or regular, and quinquifid, or 5, 10 toothed or 2 lipped, the lips entire or divided. Corolla monopetalous, hypogynous, tubular, irregular. Upper lip undivided or bifid, in æstivation overlapping the inferior trifid one, stamens 4, digynamous, two of them sometimes sterile filaments inserted alternately with the lobes of the lower lip; anthers 2 lobed, the lobes often divaricated, sometimes 1 celled, ovaries 4, 1 seeded, connected at the base by means of the style, situated upon a glandular disk, ovules erect; style 1, originating from the receptacle; stigma bifid, usually acute. Fruit of four achenia or small nuts, enclosed in the persistent calyx, one or more not unfrequently abortive, albumen little or none. Embryo erect; cotyledons plane. Herbacious plants or undershrubs, stem 4; cornered, with opposite ramifications, leaves opposite, divided or undivided without stipulæ, replete with receptacles of aromatic oil. Flowers in opposite, nearly sessile cymes, resembling whorls; sometimes as if capitate."

Now, we ask, is not such a lengthened description as this sufficient to disgust any beginner, particularly when we might equally

well distinguish the particular group by the following short differential character:

Labiatæ.—Monopetalous dicotyledons, with ringent corollas, and superior gynobasic ovary.

It may be urged that such a description would approach to an artificial classification, inasmuch as it places the distinction on the modifications of a very few organs; but such a course would not only be warranted by very high example, but we also unesitatingly assert, with all deference to Mr. Mackay, that it would be perfectly in accordance with the true principles of natural classification; for, have we not mentioned the predominating characters, which are as necessarily co-existent with the minor details of organization, as the hoof of a cow is with her stomach.

The only thing which, it appears to us, could excuse the insertion of these tedious descriptions in a practical Flora, would be their accurate correctness. If the language employed was in perfect accordance with the improved glossology of the present day; or if the description of organs zealously inculcated their true nature; or if particular care was taken to point out all the exceptions to the general rule, we might forgive the difficulty and tediousness of these descriptions. These subjects are in general rather carefully attended to, but we have met with some omissions which ought not to have been allowed. Thus in the compositæ, the anthers are stated to be united, without mentioning the burreed as an exception. This, in a differential character, would have been of no consequence; but in a descriptive it is a serious error.

Mr. Mackay appears to have been himself aware of the difficulty of employing the natural system according to his own enunciation, and he has, as we before stated, subjoined a synopsis of Irish genera, arranged according to the Linnæan scheme. He has, in the execution of this, we conceive, committed a grievous practical error, in not making a synopsis of the extremely difficult genera of the umbelliferæ; the distribution according to the arrangement of Koch or Decandolle would have been desirable, although it is extremely likely that Mr. Mackay, with his vast practical experience, might, if he had applied himself to the subject, have discovered a much better method than either; but as the Flora is at present constructed, the student is allowed to flounder from genus to genus, in a family which, with every possible assistance, is still difficult. It is evident from what we have said, that we do not entirely approve of the general plan of the first part; but we are rejoiced at being enabled to express nearly unqualified approbation of the details; the distribution of the genera in the cruci-

feræ, caryophillaceæ, leguminosæ, rosaceæ, and compositæ, is admirable, greatly facilitating the study of those extremely natural and consequently difficult families. The genus salix is rendered actually easy by the excellent arangement of its species. A vast quantity of minute observation and discovery is revealed, not only on the part of Mr. Mackay himself, but also of a great number of his friends. Every one has heard of Mr. Mackay's discoveries in the genera saxifraga and erica. It might perhaps be invidious to allude to any one of Mr. Mackay's numerous contributors in preference to the rest; but he has himself especially acknowledged his obligations to Mr. Moore of the Ordnance Survey, the discoverer of pyrola secunda, rosa sabini, and carex leuxbaumei. Mr. Mackay must excuse us if we point out what we consider omissions even in this department. Why did not he unite the genera conopodium and carum, according to Decandolle's example, genera which have not a single diagnostic character to distinguish them? and why did not he change the vile Linnæan names, calyx and corolla, in reference to the floral envelopes of the gramineæ? Whether we do or do not agree with Mr. Brown, in supposing the latter of these to be formed of three whorled pieces, surely we must all coincide in condemning the use of the word calyx in reference to the bracteas, placed at the base of the locustæ. The continuation of these terms, was particularly objectionable in a Flora arranged according to the natural system. If it were possible also to have combined the dichotomous method pursued by Lamarck, in the "Flora Francaise" of 1778, with the natural arrangement adopted in the present work, it would have been desirable. We make these remarks in the purest spirit of anxiety for the success of the work; we are desirous that our Irish Flora should yield to none in accuracy or facility; it is quite possible that our opinions may be erroneous, we suggest them merely for the consideration of future editors.

We now proceed to an examination of the second part, including a description of Irish mosses, liverworts, and lichens, by Dr. Taylor, the well known coadjutor of Sir W. J. Hooker, in the Musci Brittanici; who, as Mr. Mackay truly says, of all the botanists of Ireland, was best qualified for the task. This gentleman has pursued in his classification of the musci, the same arrangement as in the work already referred to.

He has introduced a new species belonging to zygotrichia of Bridel, a genus closely allied to tortula, and forming an interesting transition between it and cinclidotus. The arrangement of the genera of the hepaticæ differs from that adopted by Hooker. Dr. Taylor has in the first place divided according to

the presence or absence of elateres among the sporules; and secondly according to the existence or non-existence of common female receptales. If Dr. Taylor intended this for a natural classification, we think it objectionable; it separates the genera anthoceros and targionia, so remarkably allied by their exserted capsules, furnished with calycels, and destitute of calyptra; and also divides the marchantia from the jungermanniæ, where the presence of calyptra appears to form an important bond. Dr. Taylor's neglecting the calyptra in his classification, is the more remarkable, as he recognizes its consequence in a physiological point of view. We find that he has reinstituted the old genera fegatella and lunularia; he has also created Marchantia irrigua, discovered by Mr. Wilson near Killarney, into a new genus, "hygrophila." We doubt very much the wisdom of using abortive organs like the so-called male receptacles, as a means of distinguishing genera, and we think it especially injudicious in the present case, where there is a liability of fostering what we conceive to be an erroneous hypothesis, with respect to the sexual function of these organs. As might be expected, the notes prefixed to the specific distinctions in this family, are exceedingly interesting, revealing a mine of long hoarded observation, and displaying vast critical sagacity. We are very sorry that Dr. Taylor has adopted the Acharian distribution of the genera in the family of the lichens; that chosen by Hooker, is certainly preferable; but Frie's classification in "Lichenographia Europea Reformata," is without doubt superior to all others. In fact it appears to be a great error, founding the classification of the lichens, especially on the nature of the apothecia; the organs of reproduction appear to be of very minor importance; this function can as well be performed by the gongyle of the thallus. The essential part of a lichen appears to be the thallus, and on the characters drawn from that we are most disposed to rely. The great diversity this organ presents, both in direction of growth, and stratification of structure, appears to point out facilities for the subdivision of this group. Nearly all our knowledge of the Irish species is due to Messrs. Templeton and Taylor. There are a great number of new species introduced into the present work. The third part of the work treating of the algæ, has been executed by Mr. Harvey, formerly of Summerville near Limerick, who is now at the Cape of Good Hope. This gentleman, it may be recollected, principally arranged the algæ for Hooker's British Flora; and for zeal and extensive knowledge of this division of vegetables, admits but of few equals. He has divided in the present work the algæ into three groups, founded on the colour of their sporules; melanospernuæ, rhodospernuæ, and chloros-

permæ. We are doubtful of the success of these changes. Mr. Harvey's reputation was too well established to require what eclat might be gained by unnecessary alterations of system; and we are afraid that these are not only unnecessary but injudicious. In a practical point of view it renders the distinction of the genera more difficult than by Dr. Greville's method, and we are not sure that the present arrangement is more natural; we might object to it on the same principle as we blamed Acharius' classification of the lichens. But taking another position, surely the colour of an organ is one of the least important points of view under which we may consider it. It is objectionable as a means of distinguishing even species in the higher orders, and botanists are obliged to use it among the algæ and fungi only for want of better characters. Speaking of taste, colour, odour, &c. Decandolle says; "n'ont d'autre importance taxonomique que de servir d'indices de certaines particularités de structure anatomique qui nous sont souvent encore inconnues." Nothing could justify the employment of such a character except the constant co-existence of a general resemblance in the details of organization. Now, can it be said that such is the case here? what, to have placed in the same group the membranous ulvaceæ, the articulated conferva, the brittle diatoma, and the gelatinous palmella? Mr. Harvey may recollect, that the common sense of Aristotle led him nearly to the same classification as the science of Cuvier, but that neither common sense nor science could convert a plucked goose into a man. In the details of this section, we have repeated evidence of Mr. Harvey's critical acumen and diagnostic accuracy. Ireland owes the discovery of a vast number of species to his exertions. His anatomical researches likewise claim eulogium.

We have now finished our review of the "Flora Hibernica;" we have not hesitated to mention every thing we disliked; we hope we have done so without exciting any unpleasant feeling on the part of the authors. It is to them we have directed our notice, and consequently have not pointed out a moiety of the things we liked. If we had done so, we are afraid we should have quoted the greater part of their book.

JOHN ALDRIDGE.

Neue Untersuchungen über die Organische Elemente der thierischen Körper und deren Zusammensetzungen. Von Gottfried Rheinhold Treveranus. Bremen, 1836.

New Researches on the Organic Elements and intimate Structure of Animal Bodies. By Gottfried Rheinhold Treviranus. Bremen, 1836.

ANALYSIS AND OBSERVATIONS. BY DR. GRAVES.

This volume, of 123 pages, 8vo., contains much novel and interesting matter. I shall endeavour to state the conclusions Treviranus arrives at, leaving the details of his experiments, and of the precautions he took to insure accuracy, to those who may feel inclined to refer to the work itself. The omissions will be less sensibly felt, should the work, as it well deserves, be translated into English; indeed I cannot avoid expressing the hope, that some one may be induced, ere long, to present this truly valuable treatise to the public in an English dress.

Having detailed in the preface the various expedients which were employed to render the microscopical examination of the tissues free from error, the author proceeds to detail in the first chapter his observations on the intimate structure and ultimate organic elements of cellular membrane. The result at which he arrives is, that in the vertebrated animals, cellular membrane is composed of cylinders or tubes, which he terms elementary cylinders. In the following instances he found the diameter of these cylinders and of the globules of the blood, expressed in decimals of the French millimetre, to be

	Cylinders.	Globules.
" Human Male,	0,002	0,004
Rabbit,	0,0011	0,0049
Hen,	0,003 to 0,004	*Greater Diameter.* 0,01; *Lesser Diameter.* 0,006

From this it appears that the diameter of the elementary cylinder is in general greater than that of the globules of the blood; I say generally, for some exceptions are mentioned, e. g. the tortoise and the frog. The elementary cylinders of cellular membrane are no doubt organized tubes, formed by a proper tunic, and we shall hereafter see that they are of great importance, not only being the groundwork of cellular tissue, but also as forming a system continuous with the lymphatics, and probably extremely influential in the circulation. For some valua-

ble remarks on the microscopical observations of Mascagni, Milne Edwards, and others, I must refer to the treatise itself.

According to Treviranus, the cerebral mass, both cortical and medullary, consists of hollow cylinders containing a soft pulpy matter. These cylinders, extremely minute in the cortical substance, are somewhat larger in the medullary, and still larger in the nerves. He does not determine whether this enlargement be owing to an absolute increase of size in the same cylinders, or to their being joined by other cylinders, which thus coalesce into bundles; the latter, however, he seems to think the most probable supposition. In following the nerves towards their periphery, he found that they have a tendency to subdivide again; and he was able to prove, that in some parts at least of that periphery, the final nervous ramifications consist of cylinders derived from a continual subdivision of the larger nervous tubes into their primitive elementary cylinders. Thus, in the retina, Treviranus has shown, beyond the probability of error, that the following is the mode in which the nervous matter is disposed: after the optic nerve has penetrated the sclerotic and choroid, its cylinders or nervous tubes spread themselves out in every direction, either singly, or collected into fasciculi; each single cylinder and each fasciculus, on arriving at a certain point, quits its former course, and bends inwards towards the opposite and inner side of the retina; immediately on making this deflection, it passes through the meshes of the vascular network derived from the central vein of the optic nerve. Before arriving at the inner surface of the retina, it passes likewise through a second vascular network derived from the ultimate ramifications of the central artery of the retina. Each nervous cylinder, or tube, obtains at this place a sheath-like covering from the vascular coat of the retina, *and thus enveloped terminates in the form of a papilla behind the vitreous humour.*

To ascertain the reality of this structure many precautions must be used, all of which are minutely detailed by Treviranus. As very fresh eyes only must be employed, these investigations will not always lead to very satisfactory results; the cylinders or tubes alluded to, lose their configuration very soon after death, and are resolved into minute globules. The papillary structure of the inner surface of the retina is consequently difficult to be verified in the human eye, unless the observer be very much accustomed to such investigations. Treviranus asserts positively, that the medullary or nervous layer of the retina is not prolonged, as some have believed, to the zonula. Near the border of the medullary lamina of the retina, he could distinctly

perceive that it is covered by two membranes, the external homogeneous (membrana Jacobi) and the internal vascular. Both these coverings extend beyond the nervous lamina, and coming into contact with each other, acquire longitudinal plaits, and proceed between the vitreous humour and the choroid in the form of corpora ciliaria to the zonula. Between these there is no trace of proper medullary or nervous substance, and those who believe they have seen such, have been deceived by the white appearance of the vascular lamina. Treviranus remarks, that it would be in fact quite useless for the medullary or nervous portion of the retina to be prolonged quite beyond the limits of the space which lies within the reach of the rays of light.

The papillary termination of the extreme nervous cylinders which compose the retina, is confirmed by a similar distribution of the other nerves devoted to the senses, e. g. the acoustic and olfactory. In order to examine this structure of the retina, it is necessary to select a clear day, when having taken small bits of the retina of an animal recently killed, and cut off with an oblique edge, unmoistened by water, and uncleansed from the particles of vitreous humour that adhere to their surface, you must place them on the object-glass of a compound microscope, which magnifies 300 times, gives an image of great clearness, and possesses a well illuminated field of vision. On the outer surface of these bits of retina, one may distinguish the radiated course of the medullary cylinders, and on their slanting edges may be seen the inward progress of the same, together with the regularly arranged, though crowded papillæ which cover the inner surface. The vascular network and the sheaths of the papillæ cannot be accurately observed on such fresh bits of retina, and consequently, to distinguish these, a particular mode of dissecting the retina, previously hardened by spirit of wine, is recommended by Treviranus. He gives the following measurements of the semidiameters of the papillæ in different animals, expressed in decimals of a Paris line:

"Frog	0,0014
Tortoise	0,0014
Starling	0,0004
Swan	0,0008
Rabbit	0,0007
Hedgehog	0,0002
Man	0,0006"

These semidiameters, observes Treviranus, obtained by means of the most accurate micrometrical measurement, are in

general several ten thousand parts greater than the number which I have already proved to represent the maximum which the radius of the image of a luminous point on the retina must possess, in order to excite the sensation of one indivisible object. Now we cannot with certainty affirm it to be requisite for clear and distinct vision, that the concentrated rays from such a point should strike the retina only on one papilla; such, however, is probably the fact.

Each papilla is convex, and undoubtedly most sensibly affected, when the middle of the image, or that part where all the rays meet, coincides with the centre of the papillary elevation; when the extremities of two pencils of rays impinge on one and the same papilla, they will not be both as distinctly seen, as if one alone fell upon that papilla. We may therefore assert, that we are not very far from the truth, in estimating the radius of the image of a physical luminous point on the retina as varying, when distinctly seen, within the limits of the decimals 0,001 and 0,0001, the Parisian line being unity. This inference, drawn from the size of the papilla, agrees pretty well, although not exactly, with the results of optical calculations made by Treviranus, and of experiments performed by Mayer. It is worth noting, as connected with this part of the subject, how much more visible a black object on a white ground is, than a white object on a black ground at the same distance. Thus, a white square, whose side equals a Parisian line, when fixed on a black ground, becomes invisible at the distance of about twenty-seven and a half inches when looked at in a weak sunshine; by ordinary daylight it ceases to be visible at twenty-four inches. Now a black object of the same dimensions, on a white ground, can be seen under the same circumstances at double that distance, viz. forty-eight inches.

Although it is probable that the papillæ of the retina, like all other nervous papillæ, are capable of becoming turgescent when their nervous energy is excited, yet their extreme minuteness prevents this turgescence from being of an amount capable of exerting any appreciable influence in diminishing the distance between the seat of the image on the papillæ and the lens. When we look attentively at an object it may become clearer, and may be actually seen with greater distinctness on account of this turgescence. Treviranus thinks that when both eyes are turned to an object with attention, it will be seen with greater distinctness for two other reasons also; first, because under these circumstances the image falls exactly at the extremity of the optic axis in each eye, and, secondly, because the iris of each eye adapts itself to its task by regulating the size of the pupil, so as to accommodate it to the distance of the object, and the

angle which the extreme rays from the object make with the axis of vision.

Treviranus's researches concerning the ultimate structure of muscles, tendons, fibrous membranes, &c. are extremely interesting, and the discoveries he has made concerning the presence or absence of the elementary cylinders, their size, appearance, and arrangement in the different tissues of the body, tend to throw much light on their individual natures as well as on their mutual relations. These matters, however important, I am reluctantly obliged to pass over in silence, in order to leave room to dwell particularly on his seventh chapter, which treats of the capillary vessels, and the roots or origins of the lymphatics.

"It has been believed by some, that the capillaries consist merely of passages percolating the cellular tissue, but destitute of any proper tunics. The following questions naturally occur concerning these vessels. Have the capillaries proper tunics? Is their diameter large enough to admit the entrance of red blood? or if their trunks are sufficiently large, have they ramifications too small for this purpose? What is their connexion, or have they any direct communication with the vessels carrying red blood on the one hand, and the lymphatics on the other?

"One may readily distinguish with a strong magnifier, that those vessels which are large enough to admit a stream composed of several rows of globules, are marked with transverse as well as longitudinal lines externally. It is not always easy to determine whether the transverse lines are not mere folds or plaits: the longitudinal are formed by the outlines of the elementary cylinders of the cellular membrane which surrounds the tunics of the vessel. The transverse lines vanish when the vessel becomes too narrow to admit more than one row of the globules of the blood. Many vessels lose at this stage their cellular envelope, and retain only a single homogeneous tunic; others, on the contrary, retain a cellular coat. The simple vessels with but one tunic occur chiefly in membranes that consist entirely of a horny material, as for instance, the epithelium, which covers the free side of the corpus ciliare, the ciliary processes and ciliary ligament as far as the edge of the cornea. Similar vessels may be detected on the posterior surface and lateral edges of the capsule of the lens, and in the pecten of birds. In these situations they sometimes occur of considerable dimensions. Thus, in the fox, this epithelium exhibits simple vessels with a diameter of from 0,04 to 0,05 (millimetre.) In the amphibia, vessels of this description may be detected in many other membranes. Thus, in the mesentery of a living frog spread upon glass, a microscope, with a magnifying power of 300, exhibits branches of veins, measuring from 0,014 to 0,028 (millim.) in diameter, which are perfectly transparent, and can only be distin-

guished at their edges, being as simple as if they were merely covered with a pellicle of pure water."

We now come to a part of Treveranus's treatise which is of peculiar importance, and which I dwell on with more than ordinary pleasure, because it contains the fullest and most satisfactory proofs of the correctness of the opinion, *that the lymphatics are the veins of the white tissues*, an opinion which I was the first to promulgate.

"In some parts of the body I observed circumstances attending these minute and simple vessels, which render it probable that they terminate in tubes of such a degree of fineness, that they are incapable of admitting the globules of the blood, and *consequently contain a serous fluid only*. This is the case with those branches of the central artery of the retina, that proceed to the posterior surface and lateral edges of the capsule of the lens. Even at their origin from the central artery, these vessels become at once incapable of carrying red blood, the very presence of which, in these parts, would be destructive of the transparency so essential to vision."

Treviranus then proceeds to detail minutely the situations in the eye, lungs, and other parts where he has observed these prolongations of the arteries containing none of the coloured blood; and indeed from their diameter being much smaller than that of the globules, it is evident that they are incapable of admitting them. He then examines their structure as compared with that of the minute lymphatics, and endeavours, I think, successfully, to solve the long disputed question as to the origin of the lymphatics, and their connexion with other portions of the circulating system. In the tortoise and cold-blooded reptiles, it is easy to make out with the aid of the microscope, the continuity of the roots of the lymphatics, with the elementary cylinders of the cellular membrane. The latter gradually coalesce, and assume new appearances as they are about to become lymphatics. Now we have already seen, that extremely minute ramifications of the arteries are the bearers of the serous fluid to the white tissues of the body, from which again the lymphatics return the colourless blood; in fact, they are the veins of the white tissues. In most parts, and this refers to the cellular membrane generally throughout the body, the blood, after being used, and before it can be allowed to join the general circulation with safety, requires to be elaborated in glands, in which a gradual change in the white blood is effected, and a gradual reunion with the red blood is promoted. In some few organs, as the brain, and the ball of the eye, no lymphatics have been

discovered. In these we may suppose that some reason exists, which prevents the white blood, after being used, from being necessarily injurious, and here, consequently, it at once mixed with the blood of the veins, and the minute transparent vessels which receive it in the first instance do not therefore undergo the changes above mentioned as preparatory to the formation of lymphatics. On the contrary, they proceed at once to join the minute veins. It is also very remarkable, that in the organs in which no lymphatic glands are found, we have but one order of tissues, the white. This is the case in the eye-ball and brain.

Many reflections arise from the consideration of this fact; but I must return to our author, who has expended much labour and research on the lymphatic terminations in the alimentary canal, with respect to which he has convinced himself that the villous flocculi, so abundant on the mucous membrane, are composed of minute tubes packed together, and continuous with neighbouring lymphatics. He thinks that some of his observations render it very doubtful, whether the old opinion, that some of the lacteal terminations have open mouths, or rather perforations in their extremities, may not after all be correct.

But not only do we find lymphatic terminations in the villi of the mucous membrane and in its general surface, but also in its crypts and follicles, which Treviranus considers to be organs of absorption as well as of secretion. To conclude, Treviranus agrees with Müller and others, that the ultimate terminations of all ducts of glands are in the form of very minute tubes, collected into lobules, and terminating each in a cul de sac; the whole mass is connected by cellular membrane, and in this way is formed the parenchyma of every secreting organ. In some there is reason to believe that the blood, from which the materials for secretion are derived, does not directly reach the parietes of the cul de sac, but only circulates in the interposed cellular membrane. In other glands, again, and the lungs present a striking example of this arrangement, the blood circulates in the parietes of the extreme ducts, which are, in the case of the lungs, the air cells. I have already mentioned that no elementary cylinders can be detected in certain tissues. It is very remarkable that this is the case with respect to the air cells of the lungs, whose tissue, consequently, does not consist of condensed cellular tissue, or, as I once thought, and as Majendie supposed, of serous membrane. With respect to the bones of man and animals, the observations of Treviranus fully establish the existence of the laminated structure. The laminæ are arranged into several layers; in the bones of man these layers are folded and multifariously perforated, which is not

the case in other animals. These folds in the laminæ, or their layers in human bones, appear in the longitudinal section to resemble fibres, for which, when examined with a microscope, they are indeed easily mistaken, and the perforations in the laminæ produce an appearance of cells. The perforations are entirely distinct from the cavities or interstices discovered in the substance of human bones by the microscope, and which are found in consequence of the laminæ not lying in complete apposition with each other, for they leave between them interstices that are filled with fluid. The researches of Treviranus, Deutsch, and Purkinjè have completely established the foliated structure of bones, and have also explained the circumstances which misled Scarpa, and induced him to form the opinion that the osseous texture is cellular. To conclude, it is necessary to observe, that Treviranus himself allows he has been anticipated in some of his investigations by the celebrated Ehrenberg, who has applied his practised powers of microscopical observation to similar researches; Treviranus, however, differs from Ehrenberg in several very important particulars, and has likewise pointed out the sources of Ehrenberg's errors. Whoever wishes to compare the results arrived at by both, may do so by means of the foregoing abstract, and of a short account of the discoveries of Ehrenberg and of Krause, translated from Poggendorf's Annalen, and inserted in the third number of Thompson's Records of Science.

Ueber die Blättrige Textur der Crystallinse des Auges, als Grund des Vermögens einerlei Gegenstand in verschiedener Entfernung, deutlich zu sehen und über den innern Bau der Retina. Von Gottfried Reinhold Treviranus. Bremen, 1836.

On the Laminated Structure of the Crystalline Lens, considered as the Means which enables the Eye to see Objects clearly at different Distances, and on the intimate Structure of the Retina. By Gottfried Rheinhold Treviranus. Bremen, 1836.

ANALYSIS AND OBSERVATIONS. BY DR. GRAVES.

This volume, consisting of eighty pages, 8vo., is of extreme interest, and calculated to confer most important benefit on the science of physiological optics.

The question, observes Treviranus, how and by what contrivances the eye is enabled to see one and the same object at different distances, without the least apparent variation either in the correctness of its outline, or the disposition of its parts; and

how the eye can even distinguish at considerable distances the shape of this object with remarkable precision; this question has excited much discussion since the time of Kepler, but has never yet been satisfactorily answered. Porterfield is the only writer who has bestowed much attention on the effects which the laminated structure of the lens must produce in changing the course of the rays of light. After much labour, and after having made a great number of mathematical and analytical calculations, Treviranus has at length succeeded in solving this important problem in the fullest and most satisfactory manner. The steps by which he arrives at his conclusions, and the difficult analytical processes necessary for the solution of each step, can be understood only by those who have studied mathematics more profoundly than falls to the lot of most medical men; I must, therefore, necessarily confine myself to a condensed summary of his views and conclusions, leaving the mathematical details to the consideration of persons who have devoted themselves more particularly to the science of optics, and recommending them strongly to bestow upon this work the attention which the talents of the writer, and the novelty and importance of his views, justly claim.

The first chapter is devoted to the solution of the question, what course does light pursue through a transparent globe consisting of strata increasing in density from the surface to the centre. The number of laminæ in the human lens must be estimated at more than 1500, and the thickness of each at about 0,0005 of a line. Treviranus proves that the course of a ray of light through the lens, must be necessarily a curve; and he investigates at great length the nature of this curve, and the consequences which result from the different curves which different rays must necessarily describe, according as they are incident on the lens at different angles, and enter it at greater or less distances from its axis. These investigations have enabled him to arrive at some very interesting results, one of the most important of which may be stated thus in general terms: it is well known that the rays of light proceeding from a luminous mathematical point, are refracted by a double convex lens, in such a manner as to form an image of that point; now, when the spherical surface turned towards the luminous point is tolerably large, as for instance, the crystalline lens, the image formed by the converging rays, after leaving the lens, is not a mathematical point so long as all those rays enter into its composition; it is in fact a circle, and is termed the circle of smallest diffusion. Now it is of great importance to consider the manner in which light is distributed over the surface of this circle of smallest diffusion; for as the circles representing the

different adjacent points of distant object, must necessarily interfere with each other by overlapping at their edges, it becomes a matter of interest, to ascertain whether much light falls on the circle towards its circumference. This interference must give rise to an indistinctness of vision, proportioned to the number of points whose circles of diffusion interfere ; that is, to the area of these circles, provided that the light is uniformly diffused over them, but if it be very rare at the circumference, the impression made by the circles belonging to the adjacent parts must be less sensible, that is, their interference with each other (technically termed the spherical aberration) will produce less indistinctness of vision. Accordingly, Sir Isaac Newton, supposing that the light is incomparably rarer at the surface than towards the centre of these circles, was of opinion that this interference could not give rise to much indistinctness. This opinion seems to be also held by Treviranus. In page 30, he says :

" That an homogeneous sphere or lens forms a circular image of a point, over which the light is very unequally diffused, and towards the circumference of which so much the fewer rays go, the more the circle becomes enlarged.

" The laminated or stratified sphere or lens, on the contrary, has certain limits beyond which it refracts the rays in the same manner as the homogeneous lens, but within which it effects a much more equable diffusion of light, by transmitting to the focus the rays in the same order in which it receives them from the luminous point. The result of this property is, that the image of the point is more accurately illuminated to the very edge of the circle of smallest diffusion."

If I had not the most implicit reliance on Treviranus's superior accuracy, I would be almost tempted to believe that on this point he had been misled by Newton, and that he was not aware that in 1767 Boscovich corrected Newton's error, and by a very beautiful analysis, shewed the distribution to be very different from what Newton stated; and that the light is extremely dense at the borders of the circle of smallest diffusion, as well as at the centre ; and that there is a space between them in which it is much rarer. As far as I can understand Treviranus, he appears to have succeeded in proving, that in the stratified lens the circle of diffusion is more equally illuminated throughout, and what is of great importance, that the circle is of smaller diameter, and consequently the image produced partakes less of the spherical aberration. With respect to the position of the luminous point at different distances from the eye, Treviranus has demonstrated, that it is owing

to the stratified structure of the lens, combined with the power which the pupil possesses of enlarging or contracting according to the distance of the point, that we have an image of the point formed, always equally well defined, and always nearly at the same distance from the posterior surface of the lens, or, in other words, in contact with the retina.

I am not prepared to say how far his analytical reasoning may be approved of by mathematicians; but his investigations seem to demand their most attentive consideration, and I shall, therefore, willingly consign to more competent persons, the thorough sifting of the calculations which he has made with reference to the solution of this most interesting problem, contenting myself with the humble merit of being the first to draw the attention of British opticians to his labours. I would also invite the special notice of physiologists to his observations on contraction and dilatation of the pupil, and to the explanation which he gives of a question hitherto unexplained, viz. how it happens that the eye can distinguish plainly, and with equal precision, an object at eight or ten inches distance, placed in lights of different degrees of brightness, although the pupil varies in size, according to the intensity of the light.

SCIENTIFIC INTELLIGENCE.

New Operation for the Prevention of Prolapsus Uteri.—Practitioners are aware of the great difficulty they experience in treating this affection. The horizontal position, astringent injections, pessaries, &c. have been recommended, and more recently, Dr. Hamilton of Edinburgh has advised another mechanical contrivance for preventing the prolapse; it consists of either a strong T bandage, or in more serious degrees of the disease, a circular metallic belt like that of the common truss, provided with a cross or perpendicular strap, and to this strap is attached a cushion stuffed with horse hair, about six inches in length by three in breadth; and a thickness proportionate to the degree of relaxation, and therefore of the support required. This bandage is to be worn whenever the patient is out of bed, and as long as any symptom of the disease is perceived. It effectually relieves the unpleasant feelings, while it enables the patient to take walking exercise, which is so essentially necessary to the relief or cure of the disease. By this simple means, in conjunction with the use of cold bathing, and of appropriate constitutional treatment, Dr. Hamilton has for many years quite superseded the use of pessaries.

Professor Hamilton formerly suggested the propriety of endeavouring to relieve very bad, and confirmed cases of prolapsus uteri, by means of exciting adhesive inflammation in the vagina, so as to agglutinate its opposing surfaces. That adhesions strong enough to prevent the prolapse of the organ might be thus formed, is proved by the fact mentioned by Dr. Collins, that he had in four cases met with adhesions of the vagina produced by former inflammation, so strong that they completely prevented the descent of the head of the child during labour, forming an unyielding band across the passage, and rendering it necessary for the accoucheur to divide the adhesion with a probe-pointed bistoury. Such adhesions, no doubt, would prove a radical cure in cases of prolapsus uteri, but the objections to any method we could devise, in hopes of producing them, seemed so great, that Dr. Hamilton abandoned this proposal as impracticable. It was argued, however, by some German surgeons, that although we cannot produce vaginal adhesions consistently with the safety of the patient, yet we have it in our power to produce adhesion between the external labia pudendi, an expedient quite as effectual

as the other in preventing the descent of the womb; nor has this reasoning been unsupported by experiment, as is proved by the following very curious case, with the particulars of which the English medical public ought to be made acquainted, on the principle that it is our duty to know every thing that has been attempted, even though we may not deem the attempt judicious, or worthy of imitation.

A woman in Hamburgh, 36 years old, and who had not been pregnant for many years, was exposed to much fatigue, and to much going up and down stairs in the house where she was in service. She perceived in 1831, a bearing down of the uterus, which gradually increased to a *prolapsus uteri et vaginæ completus.* She was treated in the hospital with injections, and afterwards with pessaries; the latter could not be managed so as to produce the desired effect, for they were either not retained *in situ*, or they gave rise to much additional irritation. Seeing that all the usual remedies had failed, Dr. Fricke had recourse to the following operation, which he has often employed successfully in similar cases. A portion of each external labium towards its edge, and on the inside, is sliced away. The breadth of the slice above should equal that of the finger, below it may be narrower; the cut parts are then brought into apposition, and secured by means of ten or twelve stitches. Sometimes a perfect union down as far as the frenulum may be effected, but in other cases, as that now under consideration, an opening between the labia remains near their commissure, while a band of union exists in the middle, quite sufficiently strong to prevent prolapsus of the uterus.

The operation in this case perfectly succeeded, but in the course of 1834 the patient married, and soon became pregnant; this caused no small anxiety on her part, for she was afraid that the birth of the child would be accompanied by the destruction of the band of flesh, to which she owed the prevention of the descent of the uterus. She accordingly consulted Doctors Fricke and Platt, who, on inspection, ascertained the state of the parts to be as follows:—The external labia were united nearly in the centre, and at the point of union exhibited a large, solid, and protuberant mass of smooth flesh, bounded both above and below by two circular openings leading to the vagina. The lower opening was about two inches, the upper one inch and a half in diameter; the bridge of flesh intervening between these openings was about one inch and a quarter in breadth, and one-eighth of an inch in thickness. Both openings were filled by a confused mass of vaginal integuments pushed forcibly downwards, and owing to the pressure of which, the lower opening had latterly become much enlarged. It is to be observed that impregnation took place through the upper opening. At the end of the usual period of utero-gestation, this woman brought forth a full grown child *which passed through the lower opening;* no other assistance was required than three incisions made by Dr. Fricke in the fleshy bridge above spoken of, for the purpose of enabling it to yield more, so as to allow room enough

for the passage of the child's head. After delivery the woman was confined to bed for several weeks, the incisions were suitably dressed, a T bandage with a compress worn, and finally, *mirabile dictu*, she arose with the adhesion between the labia as strong and perfect as before delivery.—*Zeitschrift fur die gesammte Medicin.* B. ii. H. ii. Hamburgh, 1836.

I have, of course, myself no experience on the above subject: I may remark, however, that the proposal for producing adhesions between the opposite surfaces of the vagina, has been several times successfully carried into actual operation. This method succeeded in Dublin, in the hands of my friend, Dr. Ireland, who has published the account of the operation in the sixth volume of this Journal, p. 484.

A similar operation has been several times since performed by Velpeau, Boivin, Laugier, and others. Some produce adhesions between the opposite surfaces, by means of wounds made with the knife; others by means of wounds resulting from the application of escharotics.

Dr. Ireland seems to attribute the merit of devising this operation to Dr. Marshall Hall, but it is probable that *Gerardin*, who proposed it in the year 1823, has the claim of priority. For an historical account of this operation, I must refer the reader to the Annali Universali di Medicina, edited at Milan by Omodei, p. 574 of the number for December, 1835. The same Journal contains an excellent translation of Dr. William Stokes's Lectures on the Practice of Physic; lectures which I am much gratified to find, have been also republished at Leipzig in German, and in English in America. R. J. G.

Formula for the internal Administration of Iron, by Meurer.—The following formula, though much recommended by chemists, has been little employed in medicine. It is an advantageous substitute for chalybeate waters, which are less efficacious than it, and much more expensive.

℞ Sulph. Ferri Chryst. ʒss.
Sacch. alb. ʒiss.
Tere simul et divid. in chart. xii.

℞ Bicarb. Sodæ. ʒss.
Sacch. alb. ʒiss.
Tere simul et divid. in chart. xii.

Dissolve one packet of each separately in water, mix, and let it be taken as an effervescing draught. The decomposition is as follows: 1st, carbonate of the protoxide of iron; 2nd, sulphate of soda; 3rd, a little undecomposed carbonate of soda, the quantity of bicarbonate being more than sufficient for the decomposition of the sulphate of iron.—*Summarium des neuesten in der Heilkunde.*

Indigo in Epilepsy.—The Physicians of the Charité in Berlin

have used indigo in many cases of epilepsy, and not unfrequently with decided advantage. In other cases it failed; nor can they point out what symptoms render either its success or its failure likely; this they propose as an object of future investigation. The following is their formula:

℞ Pulveris Indigo ʒss.
——— Aromatici ʒss.
Syrupi Simplicis ʒii.
Sumat cochleare unum minimum ter in die.

This dose is gradually increased until half an ounce of indigo is taken in the day. In general the patients experience no inconvenience, except perhaps some looseness of bowels from the larger doses, and in some cases nausea and vomiting.

The stools are soon tinged of a deep blue colour, but the urine does not acquire more than an opalescent, bluish-green hue. This remedy at first produces an apparent aggravation of the disease, rendering the seizures more frequent; this effect, however, soon subsides, and is followed by a gradual but steady diminution in the frequency and the violence of the fits, until they cease altogether. We have already observed, that this only takes place in some cases. In a disease like epilepsy, it is of great importance to add to the list of remedies which are even occasionally useful, as where one fails another may succeed.

Since the above was written, I have seen the July number of the British and Foreign Medical Review, where a still later report from the Charité and also other works on the same subject are referred to. The following observations, taken from these extracts translated in the Review, are extremely interesting: —

"Dr. Roth, however, affirms, that with the greater number of patients very different phenomena are observable. In most instances a sense of constriction at the fauces, and the impression of a metallic taste on the tongue, shortly succeed the first administration of indigo, although the medicine itself is perfectly tasteless. Nausea, inclination to vomit, &c. with delicate persons even vomiting, follow in succession: at times the retching is so violent as to preclude the further use of the remedy, whilst in the other cases the nausea continues for two or three days, and then gradually subsides. The retching and vomiting produced by indigo differ somewhat from those which are the effect of common emetics; the contractions of the abdominal muscles and of the diaphragm being less violent, and the act not being succeeded by the usual feeling of distress and prostration. The egesta present no peculiarity except in their blue colour. After all inclination to vomit has ceased, a diarrhœa usually takes its place, increases rather than diminishes as the treatment proceeds, and lasts during the whole time that the indigo continues to be administered. Thus from four to six stools of a semifluid consistency and a blue-black colour are produced daily, with the accompaniment of slight gastro-enteric pains, which are likewise observed during the period of

nausea and retching, and are in rare instances so violent as to proscribe the further use of the remedy. The protracted diarrhœa ultimately brings on dyspeptic symptoms, with vertigo, &c. In one or two instances the bowels remained obstinately constipated instead of being relaxed. Indigo does not appear to increase the quantity of the urine, but invariably to tinge the latter of a dark violet hue, paler in the urine secreted at night than in that of the morning.

"Dr. Roth has never found that indigo affected the colour of the perspiratory matter. Slight spasm and twitchings of the tendons sometimes arose after the treatment had lasted for a few weeks.

"*Form of Exhibition.* The experiments were made with the Guatimala Indigo of commerce [from the Indigofera Disperma, or the Argentea.] The best mode of administering it is in the form of an electuary, in which one portion of indigo is combined with two of syrup and a small proportion of water. In the form of powder it is apt to occasion distressing spasm at the isthmus faucium. Its tendency to produce diarrhœa may be corrected by the simultaneous use of mild tonics or astringents. The pulv. ipecacuanhæ comp. was found to answer this purpose sufficiently well.

"*Dose.* Small doses appear to be wholly ineffectual. If the remedy agrees with a patient at all, it is necessary to commence with tolerably large doses, which may be gradually augmented to an ounce or even more, according to circumstances, *per diem;* the corrigents to be increased in the same proportion. The treatment it may be necessary to continue for three or even more months.

"*Effect on Epileptic Patients at the Charité.* At the commencement of the treatment, the frequency of the paroxysms was in every instance manifestly increased. Each attack was of shorter duration than formerly, but more violent; the subsequent prostration, therefore, more considerable. The premonitory and soporous stages were less prolonged than usually.

"After from one to eight weeks, according to the dose administered, all the above phenomena gradually began to abate. In the successful cases the paroxysms diminished in frequency, duration, and violence, and the last attacks amounted to no more than slight shiverings and inconsiderable twitchings.

"The total number of cases of epilepsy treated with the indigo was twenty-six. It has already been stated that only those were cured by it whose epilepsy was idiopathic. The number of cures witnessed by Dr. Roth amounted to nine males and five females. Eleven others were relieved, whilst in six cases the complaint underwent no improvement. Dr. R. was particularly struck with the instance of a boy, aged sixteen, who for eight years had been the victim of chorea St. Viti, to which epilepsy had supervened, but who was radically cured of this double affection by a six-weeks' use of indigo.—*Hecker's Neue Annalen. Erster Band. Erstes Heft.* Berlin, 1835."

Employment of Creosote in Pulmonary Phthisis.—Dr. Junod, d'Iverdun, has addressed a letter to the president of the Academy of Medicine on this subject. He alludes to the observations of many physicians on phthisis pulmonalis having been cured or advantageously modified by patients inhaling tar vapour; and thinks it likely, that the less frequent occurrence of phthisis in seaports is attributable to the effluvia of tar which prevail there; and goes so far as to suppose that the benefit derived from sea voyages by phthisical patients is owing to the inhalation of the vapour of tar in the vessel. After alluding to the process of heating tar for the purpose of disseminating the vapour through the patient's room, as practised by some physicians for the above object, M. Junod proposes the following method as possessing the advantages of procuring vapour more rare and of more equal strength.

A flask furnished with a loose glass stopper, and containing creosote, is the simple means that he recommends. It will be enough to place it near the patient's bed, as it will give sufficient odour even without withdrawing the stopper. In case it may be necessary to render the effluvia stronger, a bit of linen soaked in creosote, and left near the patient, will answer the purpose, and the dose may be graduated by diluting the creosote more or less, according to circumstances.

M. Junod states that he has obtained beneficial results from this practice, and that it may be more useful for those labouring under phthisis to publish this observation, in order that practitioners may take it up, than to wait until he had completed his investigation on the subject.—*Rev. Med.* May, 1836.

Manner of obtaining Blood in Cases where the Vein does not yield it readily, by Dr. Burdach.—The plan is applicable to all cases where open veins do not give a sufficient quantity of blood, or in bleeding fat persons where the veins are not very apparent. To produce the effect, says M. Burdach, it is merely sufficient to apply a ligature also on the other arm, as if you were about to open a vein in it. After an interval of from two to ten minutes, the vessels of both arms will be swollen and full of blood. As soon as the person feels numbness, the ligature is to be relaxed, and compression made with the thumb, that the blood of the open vein may flow in a jet; the flow of blood is to be kept up or stopped by tightening or relaxing both ligatures.—*Gräfe et Walther Journal der Chirurgie.*

Pneumonia in Children treated by White Oxide of Antimony, by M. Constant.—The white oxide of antimony is now-a-days the most powerful means of treatment in thoracic inflammations of children. It is in daily use by M. M. Baudelocque and Guersent. The latter gentleman never exhibits it until after bleeding, either generally or locally, and often applies blisters to the chest in the course of treatment. M. Baudelocque, administers the oxide in a far greater number of cases, and confines himself to the

use of this preparation only, in the treatment of very young children, those who are very delicate, scrofulous, or who have been weakened by former diseases. He never has recourse to blood-letting, unless in intense pleuro-pneumonia, attacking children of more than seven years old, and who enjoyed fulness of strength at the time of the attack. M. Constant has several times observed the resolution of pneumonia to occur during the exhibition of the white oxide of antimony. We lay the more stress on this fact, as some practitioners, those who are in favour of repeated blood-letting, state, day after day, that the oxide of antimony has fallen into an oblivion from which it can never be rescued.

Obs. 1. Delsarte, æt. 7, of a delicate constitution, admitted to the Hospital des Enfans the 10th Nov.; had been four days suffering from cough, oppression, and fever; during the next two days diarrhœa set in, with delirious paroxysms in the evening. Decubitus on the left side, face flushed, cough small, dry, and distressing, respiration abdominal, forty-eight in the minute, dull sound on percussion, and bronchial respiration, and bronchophony in the whole extent of the left scapula above, while there was a mucous rale at the right side; the respiratory murmur was strong, and not much interrupted by rales; no pain of the side, and no expectoration whatever. The lips were red, dry, and cleft, tongue red at the edges and tip, and dry; skin hot and dry; pulse 160. Mucilaginous julep was ordered with twenty grains of white oxide of antimony. This treatment was persevered in for three days without any sensible alteration in the state of the patient; on the fourth day the dose of the oxide was increased to half a drachm, on the following day the skin was found moist, and the diarrhœa arrested; the pulse came down to 125, and the inspirations to 32 in a minute; ordered one drachm of the oxide, which was repeated on the sixth, seventh, eighth, and ninth days, during which time the skin continued moist; the respiration became by degrees nearly equal in both lungs, and the sound on percussion the same; there were only heard some mucous rales under the left scapula, the pulse 92, respirations 28: the drachm doses were continued to the twelfth day, and the child left the hospital perfectly cured in a fortnight after.

Obs. 2. Morteau, a child aged 6 years, of a scrofulous constitution, had been several months affected with an abundant leucorrhœa; when brought to the hospital, she presented the symptoms of a double pneumonia, the face was red and expressive of anxiety; intense dyspnœa, interrupted voice, dilated nostrils, frequent soft cough, without expectoration, percussion painful on both sides of the chest; bronchial respiration at the top of the left lung posteriorly, and crepitating rale at the top of the right lung; anteriorly clear sound and snoring rale, inspirations 60, pulsations 140 in a minute, skin hot. In this case the treatment began with half a drachm of the oxide, increased at the end of two days to two drachms. The symptoms gradually subsided into the following, which appeared on the fifth day of treatment, viz., mucous rale at

the right side, and moist crepitating rale at the left. The dose of the oxide was then gradually diminished to the fifteenth day, when the respirations were not more than twenty-four, and the pulse not more than eighty in a minute, with sound on percussion equally clear at both sides of the chest; the recovery was rapid.

These cases in which the white oxide of antimony was used to the exclusion of every other plan of treatment, leave no doubt as to the efficacy of this preparation in infantile pneumonia. Many other analogous cases could be brought forward. We find that the greatest number of the principal symptoms have been successfully modified by its influence. In the cases cited, we have accurately noted the evident lowering of the pulse during the use of the oxide, and the accelerated breathing has become considerably retarded under the same means. Sweating, which has always been looked on as a favourable sign in pneumonia, exhibited itself with all those patients very few days after the employment of the antimonial preparation. The action of this medicine on the digestive canal is decidedly different from that of tartar emetic. Out of eighty patients affected with pneumonia, and treated by oxide of antimony, only two had had vomiting, and diarrhœa has been observed only in very few cases, and in some where diarrhœa had existed before the exhibition of the remedy, the disappearance of that symptom was not interfered with even by augmented doses. The tongue, unless in complicated cases, has always remained full and moist. Finally, the white oxide of antimony may be looked on as a suitable remedy, 1st, in cases where blood-letting has been insufficient, and cannot be repeated without danger; 2nd, in those cases where abstracting blood is manifestly contra-indicated. As a general rule, it ought in very young children to be preferred to tartar emetic, which often causes severe symptoms at an early age.—*Rev. Med.* May, 1836.

Yellow Fever at Goree, (Senegal,) by M. Chevé.—Although much has been written on yellow fever, yet the following abstract is worthy of notice, from the mode of appearance of the fever, as well as from the vivid description by a medical gentleman who was attacked with it. The fever set in about the middle of June, quite suddenly, though the weather had been unusually calm and healthy for some days before, and there were no traces of its having been imported by any vessel entering the harbour. It was characterized by symptoms of violent meningitis at first; at a later period the skin got yellow; there was black vomiting, lacerating pain in the lumbar regions, suppression of urine, &c. At the end of thirty-six or forty-eight hours, apyrexia indicated sulphate of quinine. What is extraordinary, after it ran its course, there was no renewal of the disease from wearing the apparel of those that had died; the clothes of the deceased being bought up at auction, and worn by the buyers with impunity. The negroes of Goree, and the mulattoes suffered nothing by it, as there died of the former only six out of 5,400, and

of the latter two out of 450. But the negroes living on the sea-shore were annihilated; whole townships were swept off, both men and cattle. Of 150 Europeans, six were not attacked; 144 took the fever, of those fifty-three died. However, we shall give M. Chevé's own account of the manner he was affected, and we are certain that very few medical gentlemen could attempt to treat themselves so heroically as he did.

The day he was attacked, (July 7th,) he had been much fatigued, he had not not changed his clothes for twenty-four hours, and had been making a post mortem examination, the stench of which remained in his nostrils: towards evening he felt oppressive pains in the lower extremities, and his head was so much affected, that he had great difficulty in regaining his own house. He states as follows:—

"As soon as I got home, I took twenty-eight ounces of blood from my arm, which produced a slight faintness. I then lay down, the pain in my head, loins, and legs was terrific; in a word, I had the yellow fever. Three hours after the febrile reaction was most violent; the pulsations of the carotids deafened me. I re-opened the vein, and took away twenty-four ounces more of blood, and half an hour after I applied two hundred leeches to the temples and neck.* My thirst was excessive, and I could bear nothing but water. During the night I had two enemata administered without effect. The fever and pains went on increasing, and at three o'clock in the morning I applied two hundred leeches more to my head, and covered my feet with mustard. Next morning, at eight o'clock, there being no improvement, I bled myself in the foot to syncope, and on coming to, I had my legs covered with two sinapisms and two blisters, and took four ounces of olive oil, and an ounce of lemon juice, without any effect; in the evening I took an ounce of cream of tartar, which produced two stools. The pains and fever persisted with the same violence, and towards two o'clock in the afternoon I, a third time, applied two hundred leeches to my neck and head, which caused an abundant hæmorrhage, but without any improvement. In the evening the blisters and synapisms were removed, there was scarcely any pain felt from them, so much were the head and loins engaged.

"At nine o'clock in the evening, a prey to horrible agitation, I bled myself a fourth time to about twenty-four ounces, by pricking three veins in my hand, forearm, and bend of the elbow. This procured me an hour's comfort, but the night was no better than the preceding, and at four o'clock next morning I re-applied two hundred leeches more to my neck, and one hundred to the epigastrium, where I felt some pain; blisters to the thighs, and two more sinapisms to the legs. On the morning of the 9th, though ex-

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* It is necessary to observe, that the leeches of Senegal are excessively small, three or four being required to take away as much blood as one of our European leeches does.

tremely weak from loss of blood, yet suffering horribly from pains, I opened the saphena, which gave but little blood; I then pricked two veins in the right arm, which yielded about sixteen ounces. After this I remained sometime relieved from my sufferings, but at eleven o'clock they re-appeared with all their intensity. I then had myself put into a tepid bath, where I remained half an hour, during which time cold water was poured on my head. I derived no benefit from this. In the afternoon I re-applied the sinapisms, and though opposed by those about me, I had three hundred leeches more to my temples and eyes, behind the ears, and to my neck; the cerebral symptoms still persisting, notwithstanding the great flow of blood from the leeches, I made a sixth general bleeding, but it was the last. I was so weak that I was more than ten minutes before I could succeed in opening the vein, and I obtained but about eight or ten ounces of blood.* In the night of the 9th two fresh blisters and sinapisms were applied; next morning (the 10th July) there was some diminution of the fever, so I waited for the remission, not being able to continue the former treatment; for in sixty hours I had lost by the lancet about eight pounds of blood, twelve hundred leeches had been applied, and my inferior extremities were completely stripped of epidermis. Towards evening, I fancied my pulse was less frequent; it was perhaps an illusion, and I swallowed four grains of sulphate of quinine, which I immediately after vomited. From that moment the second period of the fever began. Through the whole night I vomited the water which I swallowed; the agitation and torturing pains returned, accompanied by a very distressing hiccup. I had a strong hope, until about midnight, when I perceived that the matter I had vomited was black as ink: this brought something of despair, for hitherto the black vomiting had been mortal. From that time not a drop of drink remained on my stomach, and once every hour I had black vomiting to the amount of a quart; it came up all at once, and without an effort. After each vomiting I remained incapable of motion for about ten minutes. On the 12th, while making an effort to relieve myself, I fainted, and remained so for a long time. On coming to, I found myself in the greatest state of prostration, and unable to turn my head; meanwhile the black vomiting continued forty-eight hours.

"In this state I influenced my attendants to apply an enormous sinapism over the whole anterior part of the trunk, and with this *cuirass* on me, I fell asleep for the first time. I slept peacefully for five hours, and on awaking, was quite free from inward pains; but the sinapisms and blisters that I had not hitherto felt, though the surface of my body was all one sore, now gave the most severe tearing. After that sleep, my convalescence began, but I was so weak that I could scarcely stir my fingers. At the end of eight

* All through the blood was thick, and scarcely formed any crassamentum, and no buffy coat.

days of the greatest care I was able to get up, and in six days after was enabled to visit the hospital, and was soon so restored, that for three months and a half I underwent the greatest fatigue and loss of sleep without inconvenience."

M. Chevé speaks of another mode of treatment, which he has found to be very successful in yellow fever, viz., keeping up permanent bleeding by means of leeches; he has tried this plan only in four patients, but with the best results in every one of them. His object is to bring about the second stage as soon as possible by this means, assisted by aperients and enemata. The first case had leeches on for thirty-two hours without intermission, (in all 900;) the second case for twenty-five hours, the third thirty-two, the fourth forty-eight hours before the symptoms were favourable for the exhibition of sulphate of quinine.

The mode of proceeding is as follows: Apply a number of leeches to the temples, nostrils, &c.; as soon as one drops off, supply his place with a fresh leech, and so on until the object is attained.—*Rev. Med.* May, 1836.

We have given in the preceding article the details of heroic depletions successfully employed in the treatment of yellow fever. Lest we should be guilty of misleading our readers, we beg to subjoin the following extract from the valuable notes published by Dr. Furlong, on the yellow fever of Antigua, in the last July number of the Medico-Chirurgical Review, edited by Dr. Johnson.

"The fever shewed all grades, from intermittent—remittent—to concentrated yellow fever, proving a common origin, the type varying according to susceptibility, or acclimated state of the individual, though not without exceptions. With regard to treatment, it was soon found that the fever did not bear bleeding; it was fairly tried by some, and given up. I never bled one of my patients, and lost only two out of fifty or sixty of all colours. I witnessed no symptom warranting the use of the lancet, though I would be far from stating that such might not have occurred in the patients of others. Calomel was the sheet-anchor, ten or fifteen grains, followed up in two hours by a dose of sulph. mag. et manæ, and then every second after, while feverish, three, five, or ten grains were given in a pill, combined with one-fourth of a grain of opium and effervescing draughts, with nitre and sweet spirit of nitre between; sinapisms to scrob. cordis, to check vomiting, were directed with good effect; if headach was severe, the hair was thinned or shaved, and cold applied, or cold douche, and sometimes it was necessary to blister the neck before relief was obtained. A tepid bath and warm pediluvia were occasionally used; the principle on which they were ordered is obvious—they tended much to relieve internal congestion. As soon as the gums were affected, the patient was generally safe, and quinine was ordered in two-grain doses every two hours. Such is a brief but faithful outline of the epidemic.

"The different opinions of writers on tropical yellow fever are

surprising, and well calculated to confound the inexperienced. One tells you your sole dependance is on the lancet—it is a *sine quâ non;* another states, after crossing the tropics, throw your lancets over board! Can such conflicting statements be reconciled? I think they may. It is well known that epidemics differ, arising from season and atmospheric constitution. The epidemic I have described occurred in the fall of the year, when all organic life appears depressed, as nature itself points out; fevers at this time of the year assume an asthenic character. It was an observation of Sydenham, Gregory, and others, that Autumnal epidemics do not bear bleeding or debilitating remedies. Fever, occurring in the Spring or Summer months, puts on a phlogistic type, at which time, bleeding and other powerful antiphlogistics are indicated, and necessary to the salvation of the patient, facts which I have many a time witnessed. It is from not marking the different seasons, that we find such conflicting views of yellow fever by authors: one sees and describes a vernal epidemic—bleeding is the sheet-anchor; true:—another Autumnal—the lancet not indicated; true also. In this way I would endeavour to reconcile such antagonizing views, though something, also, is due to the malignancy of the occasional cause, longer or shorter exposure thereto, and other circumstances, which will suggest themselves to the medical reader."

Dissolving of Urinary Calculi, by M. Bonnet, Surgeon to the Hotel Dieu, Lyons.—A year ago M. Bonnet stated that for the purpose of destroying urinary concretions by means of the voltaic pile, it is necessary that they be plunged in a solution of an alkaline salt; this salt being decomposed, and its elements attracted by the wires of the pile, placed in contact with the calculus, the latter is dissolved at the acid extremity, if there be insoluble phosphates present, and at the alkaline extremity, if uric acid or urates have been formed.

Since the time referred to, M. Bonnet has proved, that six drachms of nitrate of potash, dissolved in six ounces of water, may be injected for several days in succession into the bladder of dogs, without the animals feeling any pain from it. The animals did not even make efforts to reject the saline solution, and evinced only a slight uneasiness when the dose was carried to eight or ten drachms, beyond which he never went.

M. Bonnet has obtained on a mare, in the space of one hour, the solution of eight grains of a calculus of a triple phosphate previously weighed. A pint of water, holding in solution eight drachms of nitrate of potash, had been injected into the bladder. The pile consisted of thirty plates. The same experiment, repeated on a horse, gave a diminution of only six grains in the hour, but then the blood which flowed from the wound that it was found necessary to make, had coagulated round the calculus and prevented its dissolving.

M. Bonnet is at this moment engaged in the construction of an

apparatus capable of conducting electricity, and at the same time of renewing the injection.—*Rev. Med.* May, 1836.

Treatment of Orchitis by Compression, by Dr. Fricke of Hamburgh.—Having found local bleedings, cataplasms, frictions, &c. to be ineffectual in the treatment of inflammations of the testicle, Dr. Fricke has had recourse to compression after the following manner:

The patient is placed against a wall, or on the edge of a sofa, so as to let the scrotum hang, the hair having been shaved off. The surgeon takes the scrotum in one hand, and separates the healthy from the diseased testicle, and with the other hand stretching upwards the skin that covers the diseased organ. If the testicle be very large he has it held by an assistant, if the swelling is not considerable there is no need of assistance. In the same manner he separates the spermatic cord, and applies round it a bandage (an inch broad, and an ell long,) covered with adhesive plaster, to within an inch of the testicle, over this he applies a second bandage in the same way. These bandages require much precaution in their application, they must embrace the cord tightly, that the testicle may not slip up near the abdominal ring, particularly when its inferior extremity comes to be bandaged, which would render the operation not only painful but useless. These two bandages being applied, the application of others is to be continued from above downwards in the direction of the large extremity of the testicle, taking care that each turn of the bandage overlaps one-third of the under one; when the large extremity of the testicle is arrived at, it is no longer possible to use circular bandaging. The surgeon then seizes the part where the first turns of the bandage were applied, and applies the bandages longitudinally to the tumour, going round the bottom of the testicle, and securing above the ends of the bandage. As many are to be applied as will cover the whole testicle; this compression must be graduated, and the best proof of the bandage being applied properly, is the cessation of pain. If both testicles be diseased, one of them is to be treated in the manner above mentioned, and when the operation is completed, the second testicle is to be bandaged to the first, making the longitudinal bandages act as and upon one testicle only. After the compression is finished the patient may walk about his room. —*Zeitschrift fur die Gesammte Medicin.*

Letter on the Medicinal Properties of the Extract of Acetum Colchici.

Dublin, 6th August, 1836.

Dear Sir,—I have great pleasure in noting down the few results of my experience concerning the effects of the extract of the acetum colchici, which formed the subject of our conversation at the Meath Hospital. I first saw it recommended by Sir B. Brodie as an alterative; twelve grains of it and six grains of calomel were formed into twelve equal pills, and one given every night. I have often administered this alterative in functional derangements of the liver, con-

nected with dyspepsia, and regard it as a most efficient alterative for such cases. Having, by careful observation, ascertained that the extract (as I had seen often the acetum itself do, though very unsteadily,) promotes the secretion with great certainty, and reduces it to a natural character when vitiated, I at length excluded the calomel, and have every reason to believe with good effect, for the extract of the acet. colchici by itself has on ample trial realized my hopes of it as an alterative. I need not point out to you, how valuable a medicine must prove, that possesses all the alterative properties of mercurial preparations without their disadvantages; indeed there are many cases in which we cannot employ a mercurial medicine; this is a medicine, from which we can, I make bold to say, derive as much benefit as from mercurial medicines, in cases of sluggish action of the liver. On this principle I have employed it with singular good effect in cases of habitual costiveness, in cases where there was bilious suffusion of the conjunctiva, or in more deeply marked cases of jaundice. A few days ago, a gentleman asked my advice for nausea, furred tongue, and clay-coloured, scanty alvine dejections. He had repeatedly taken pil. hyd. and calomel, which brought away dark greenish stools in the end, but had given no permanent relief; he took ex. acet. col. gr. i. every six hours, his bowels were acted on by the medicine, and his stools were of the natural bilious brown character, and are so to this day.

Every one must have experienced how uncertain the preparations of colchicum in common use are in their action. The extract of the acet. has never failed in my hands to act upon the alimentary mucous membrane, and on the hepatic secretion; and if it be true, as some (Epps.) assert, that much of the benefit arises from this effect, (by reason of its counter-irritating,) that colchicum acts in cure of many diseases, then the ex. of the acet. from its regularity of action, is entitled to the preference. On this principle I administer it both as an alterative and purgative in combination with other cathartic extracts in thoracic diseases, where such a transfer of action or counter-irritation would be advisable, and I can safely say, that I have done much good with it in cases of bronchitis, peripneumonia, hæmoptysis, &c.

In rheumatic cases, of course, I have repeatedly employed it, and with decidedly more beneficial results than any preparation of the plant.

I beg to assure you, that though I have, at your request, hastily noticed a few of my ideas concerning the medicine in question, I have *not* hastily come to the conclusion on which I have ventured, but have been scrupulously watching the medicine in its operation for a considerable period of time, and am glad to think that I have now handed it over to one whose opportunities of putting it to the test are every way so ample, and likely to bring this valuable medicine before the profession in Ireland.

I have mentioned one grain as the dose; this may be given every night when an alterative intent is in view; it may be followed on the following morning by a magnesian draught, or a small

but sufficient quantity of ex. colocynth. comp. may be added to it to quicken its action.

In rheumatic cases, and in the inflammatory affections of the chest, as well in hepatitis, I have used a pill consisting of one gr. of the ex. acet. colchici, and two to four of the ex. col. comp.; one to be given every four or six hours, till it act freely. I think colchicum is very apt to produce alvine discharges, with more or less blood in them; a circumstance that it behoves us to bear in mind, when mucous irritation in the alimentary canal obtains.

I have to beg your apology for this hasty production, and subscribe myself with much respect,

Yours very faithfully,
ALEX. J. HANNAY.

Letter from Dr. Prichard *on the Subject of Mr. Wallace's Opinion concerning the Want of Sagittal Suture in certain Tribes of Negroes.*—The following letter from Mr. Wallace appears in the last July number of the London Medico-Chirurgical Review, edited by the Messrs. Johnson.

" 27, Surrey-street, Strand, 7th June, 1836.

" GENTLEMEN,—The following little piece of intelligence you may perhaps think worthy of being presented to your readers.

" In December, 1833, His Majesty's Sloop Trinculo, to which I was then attached, while cruizing in the Bight of Biaffra, on the West-coast of Africa, captured two small schooners carrying (both) about 100 slaves, consisting of men, women, and children, the whole of whom we landed immediately after at the island of Fernando Po. The Trinculo did not return to Fernando Po until October, 1835, at which time we naturally made inquiry after our old friends the liberated Africans, all of whom, with the exception of those who had died, we found domiciled on the island, having the character of being idle, disobedient, unkind to each other—in short, possessing a host of bad qualties, with scarcely a single good one to redeem them. But the remarkable circumstance to which I wish to call your attention is, that, of those who had died, four had been examined on account of supposed disease in the head, and in all the sagittal suture was wanting. At the first examination the circumstance did not excite any very great degree of surprise, as it was merely supposed to be a *single* deviation from the ordinary process of nature, but when a second, a third, and even a fourth, evinced the same thing, then it not only excited much attention, but produced the very natural conclusion, that in this race of blacks such is the usual cranial conformation. If such is really the case, to say the least of it, it is exceedingly remarkable; and for what purpose nature should so perform her work, is a question which, although interesting, it may not be very easy to determine.

" These blacks were principally from that part of the West-coast of Africa, lying between the rivers Gaston and Congo, and

it has long been noticed, that the slaves taken from that quarter are neither so powerfully made, nor in possession of the same degree of intelligence, as the tribes to the northward and westward; but, upon the whole, there is a fair development of skull, and, so far as I have noticed, the mechanism of the head, with the exception alluded to, is perfect.

"From Mr. Ballard, surgeon of the establishment at Fernando Po, (the gentleman who kindly furnished me with the information now given,) I received one of the skulls above-mentioned, which, since my arrival in England, I have deposited in the museum at Haslar.

"I am, Gentlemen, very respectfully your's,

"JAMES WALLACE,

"*Surgeon, R.N.*"

We have thought it right to give the whole of Mr. Wallace's letter, for the purpose of annexing the following from Dr. Prichard on the subject, and of expressing our opinion, that, in the skulls mentioned by Mr. Wallace, the obliteration of the sagittal suture must be considered as a mere accidental variety, and as it not unfrequently occurs in other races of mankind, it cannot of course be considered as characteristic of any particular tribe.

"Bristol, July 20, 1836.

"You once asked me whether I had seen wormian or triquetral bones in negro skulls, to which my reply was, that they existed in several to which I had recourse to decide the question. I never till then heard of the doubt whether they exist or not, but since have seen Berthold's dissertation. Neither Soemmering, Berthold, Blumenbach, Bory De St. Vincent, nor any writer known to me, mentions the want of sagittal suture: I do not believe it characteristic of any particular race. Many negro skulls, and some European, have the osseous structure so condensed by a more than ordinary deposition of earthy matter into the tissue which supports the bony fabric, as to be heavy like marble. In such heavy skulls *all the sutures* become evanescent. I have by me now four negro skulls. In one, the heaviest, a Creole, son of two slaves from the coast of Africa, the sutures are barely perceptible; the posterior half of the sagittal is quite obliterated: there are some wormian bones in the lambdoidal: nearly all the sutures in the face are quite imperceptible. In the other negroes, which are considerably less of weight, the sutures are all very strongly marked. In the skull of a Greek, a native of Corfu, the sagittal suture is obliterated in a considerable part of its tract. This Greek skull has a good deal of the white marble appearance with the first negro. The heaviest and most marmoraceous skull I ever saw was, one of a Mulatto, which weighed 2lb. 10oz. There are some curious remarks on this subject in Blumenbach de Generis Humani Varietate Nativâ and in Soemmering.

"J. C. PRICHARD."

Kennedy on Tartar Emetic in Obstetric Practice.—In the American Journal of Medical Sciences for February, 1836, is a paper by Dr. Evory Kennedy of this city, on tartar emetic in obstetric practice. Although we feel a little jealous at Dr. Kennedy's paper being published in the first instance at the other side of the Atlantic, yet in justice to our readers we have deemed it right to present them with the whole of his paper; for doing this we need offer no apology :—

"If it be the duty of the hospital superintendent to avail himself of the opportunities afforded him of investigating new and interesting plans of treatment, establishing what is correct and rejecting what is erroneous in practice, it is equally his duty to afford the results of his investigation and experience to the public. The following observations assume to be, not merely the result of one or two experiments or accidental cases, but the report of practice adopted and tested by the experience of years in what will be admitted a sufficient field of observation,—the Dublin Lying-in Hospital. The plan here followed of selecting a medicine and treating of its efficacy in several different diseases, may appear to some to savour of empiricism. The only motive for this variation from ordinary habit is brevity, a plea that has novelty as well as simplicity to recommend it to my readers.

"*Tedious Labour from Rigidity of the Os Uteri and Vagina.*—Some patients continue for many hours in the first stage of labour, with partial dilatation of the os uteri and external parts, in whom there may be no want of what are significantly termed grinding pains, a state more frequently met with in first pregnancies and those who marry late in life. This state is to be carefully distinguished from false labour, which it much resembles, by the partial dilatation of the os uteri, protrusion of the membranes, and presence of glairy discharge. It may continue for some hours, rendering the labour more tedious than it would otherwise have been, the parts eventually becoming relaxed, and the labour terminating favourably; it may continue for many hours, exciting our dread as to the result of the case: or, in its more obstinate forms, it may persist so long as to wear out the mother's strength in unavailing efforts to overcome the difficulty it opposes to delivery, the mother, child, or both, perhaps, falling a sacrifice. The difficulties to encounter here, are premature, too forcible or irregular uterine action, propelling the child against the os uteri before it is sufficiently dilated or dilatable, and absolute rigidity of the parts. It should be constantly borne in mind that the first inconvenience is a very frequent cause of the second. With this view of the subject, then, the two objects to be held in view are, mitigating too early, violent, or irregular uterine action when this is the cause of delay, and producing relaxation when rigidity is present. Of the efficacy of tartar emetic in producing the first effect, we shall presently treat, when on the subject of violent labour; for the present let us inquire into its utility in the second. In tedious labour, from rigidity of the uterus, the os is found slightly gaping, with a

thickened, tense state of the lips, and usually much heat of the parts. Bleeding from the arm, and on the continent the use of the warm bath have been had recourse to in these cases. Bleeding is attended with marked benefit when there is a full bounding pulse, in a strong plethoric habit: but, as a general practice, it is not unattended with inconveniences often of a very serious nature. It certainly procures relaxation of the os uteri, but along with this it may cause depression of too permanent a nature, and thus seriously interfere with the future progress of the labour. Tartar emetic solution has been successfully employed in producing relaxation of the os uteri in these cases, and possesses the advantage of being much less permanently debilitating. It is an agent by which the system can be with safety brought into a much greater degree of temporary depression; between which state and relaxation of the contractile tissues, a marked connexion holds, if they do not absolutely stand in the relation of cause and effect. The principal recommendation, however, to tartar emetic in these cases is, that in its use the power of regulating the necessary degree of lowering the system, exists completely in the hands of the practitioner, as he has only to increase, diminish, or suspend the dose, in order to produce the effect he wishes; and, when the necessary effect is produced, the withdrawal of the medicine leaves the vital energies but little impaired. The medicine has been used in the ordinary nauseating doses, as in pneumonia, five or six grains of the tartrate of antimony, dissolved in eight ounces of water, and generally twenty drops of laudanum, and a small quantity of syrup added; one, two, or more table-spoonsful of this mixture are given at intervals of from fifteen minutes to two, three, or four hours, according to the effect it produces, and the necessity that exists for bringing the patient speedily or otherwise under its influence. Sometimes it is necessary to cause free vomiting in the first instance, or the ordinary doses produce no nauseating effect; in such cases the laudanum is better withheld, but may be added afterwards if necessary. In other cases the medicine acts too violently as an emetic, or produces purging; here increasing the quantity of the laudanum, and diminishing the dose, or allowing a longer interval to intervene between the doses, will be necessary. The accoucheur must, therefore, watch carefully the effects of the medicine during its administration in every case in which it is employed; these observations applying with equal force to the other forms of disease in which its utility has been proved. Under some of the circumstances described, or where the antimonial in every dose and form disagreed with the patient, small and frequently repeated doses of hippo [ipecac.] have been substituted (three to five grains every hour or second hour,) and with good effect, not only in rigidity of the uterus, but in the other diseases in which tartar emetic was found efficacious. It should be mentioned, that neither tartar emetic nor venesection has been relied upon singly in some cases where it has been necessary to produce speedy dilatation of the os uteri, and where the plethoric state of the system described was

present. In such, after depletion, the patient was kept for some hours under the influence of the nauseating mixture. One case, in particular, of a most threatening nature, may be mentioned, in which a strong robust woman was brought into hospital with the arm forced into the vagina, through a tense, rigid, and slightly dilated os uteri. She was so treated, and with the best results. There is a somewhat different state of the os uteri, in which it occasionally dilates very tardily also; here the lip of the uterus is thin and stretched over the head of the child, not affording the sensation of heat or rigidity of fibre observed in the case above described. The extract of belladonna appeared of service in a few of these cases, although its general efficacy appeared very questionable. In two cases of rigid os uteri, in which it was freely used, its application was followed by head symptoms and depression of pulse; in one of which even insensibility and stertor were present. It was, however, tried in many other cases, without being followed by these unpleasant effects. The last described state of the os uteri is also occasionally benefited by the nauseating medicine. It may depend, however, upon other causes, not at present under our consideration, nor is it looked upon with the same anxiety by the accoucheur as a cause of tedious labour. In concluding this branch of our subject, let it not be inferred from what has preceded, that tartar emetic will invariably succeed in procuring dilatation of the os uteri; as it is in some cases found quite unavailing, in others inadmissible. Its efficacy, however, in a great many cases in which it has been used, fully warrants its attracting the attention of the obstetrician, and its success will depend much on a proper selection being made of the cases in which it is available.

Irritable or Violent Labour.—By no means an unfrequent cause of tedious labour is extreme irritability or violence on the part of the mother, a state most frequently met with in first children, and in the lower ranks, but not confined to these. The patient, from the very commencement of her labour, is restless and irritable, keeps constantly changing her posture, and, if in bed, tossing about; she soon becomes very violent and vociferous, and, when the pain is present, is absolutely uncontrollable. By this means she loses the effect of her uterine efforts—she does not wait for these to bear down, but keeps up a continued and unavailing straining at expulsion in their absence, and, when they are present, she becomes violently restless, and forcibly and suddenly inspires, in place of fixing her respiratory muscles to assist in expulsive efforts which would now prove availing. This state may continue for many hours, or even for days, with more or less complete dilatation of the os uteri, and with little or no advance of the child through the pelvis; the patient at length becomes fatigued. The irritation and restlessness continue, but the uterine efforts cease, and exhaustion setting in, we are obliged to have recourse to instruments to effect the delivery. In some, the os uteri is not even fully dilated at the period when forced delivery becomes necessary, and the crotchet is then the only instrument available. In

protracted violent labour, however, the child is dead in the great proportion before delivery, from the delay and violence of the parent. In these patients where the pulse is full and the habit plethoric, venesection is often of service. The advantage that the use of tartar emetic offered in the cases already treated of, recommend it even more strongly to our notice in this. Long continued observation of its utility in cases of this kind, and the having found it procure the double effect of dilating the os uteri and soft parts, and suspending the irritability and violence which so much interfered with the natural progress of the labour, have convinced me, that by its full administration in the cases described, we may often enable a woman to be speedily and safely delivered by her own efforts, who, without it, would have suffered from a tedious and dangerous labour, eventually, perhaps, requiring the use of instruments. To produce any good effect, it must be freely administered, the patient brought completely under its influence, and retained so whilst any tendency to irritability or violence remain; nor does it appear to suspend the labour altogether, as might be supposed; it merely mitigates or regulates the violence of the pains, and under its use the labour progresses, and the head advances into the pelvis. It should not, however, be persisted in so long as to reduce the powers of the patient too much; but should be discontinued when we have attained our object, quieting the patient. These observations will also hold as to its utility and application in the cases alluded to at the commencement of this paper, where the os uteri remains undilated from the occurrence of premature, forcible, or irregular uterine or abdominal efforts.

Puerperal Convulsions.—The efficacy of tartar emetic in puerperal convulsions is quite as marked as in the preceding cases. It should, however, be understood, that it has not been used to the total preclusion of bleeding, which must always prove our sheet anchor in this violent disease. It renders unnecessary the repeated bleedings we have hitherto been obliged to have recourse to, as the only effectual means of checking or preventing the repetition of the fits. After one copious bleeding, the tartar emetic is to be freely administered, as already explained, and the patient kept well under its influence. The return of the fits will, by this means, in the great majority of cases, be prevented; and even in the most obstinate cases, they will be lessened in their severity and frequency. Its administration must not be desisted from until the patient is delivered, unless it produces too great prostration, even in diminished doses; neither is its use to preclude a repetition of the venesection, if symptoms should be sufficiently urgent to require this; nor the use of such other local or ordinary means of treatment as are generally had recourse to. There are cases of puerperal convulsions, in which neither this nauseating plan of treatment, nor free depletion, afford the benefit generally derived from them; on the contrary, the disease appears to be aggravated by these. A remarkable case of this kind occurred lately in the hospital, in which, after the patient undergoing the ordeal of

depletion, the nauseating mixture, as well as purgatives, shaving and cold applications to the head, sinapisms to the feet, blistering, and so forth, the disease was becoming more and more violent under their use, when she was put freely and rapidly under the influence of opium, and with almost magical effect. It would, however, be wandering from the original plan of this paper to go further into the subjects treated of, than in connexion with the use of tartar emetic in them; we shall, therefore, only add, that this medicine is admissible and eminently useful in all cases of puerperal convulsions, in which depletion proves serviceable.

"*Obstructed and inflamed Mammæ.*—There are two distinct states of the mammæ occurring after delivery, which demand our notice. In both, pain and tenderness, tumefaction and hardness are present; but one is an inflammation, the other merely an obstructed state of the organ; though the latter generally precedes and may produce the former, yet it does not so necessarily in all cases. There is no case within the range of obstetric practice so clearly under the control of the medical attendant, as the tumified, lobulated, and obstructed state of the mammæ, and none so sure to terminate unfavourably if improperly managed. To treat it with effect, it is necessary to look to its cause. The immediate state is a vascular determination to the glandules, together with an accumulation of the lacteal secretion within the ducts and tubes, and often infiltration into the connecting cellular tissue; a state most frequently arising from a disproportionate action between the secretories and excretories. We can easily understand the frequency of the occurrence of this state of the breasts, when we consider how suddenly in most cases determination occurs to them after delivery. Let us, then, bear in mind the function the glandular part of the organ is so suddenly called upon to assume, and this, perhaps, for the first time: thus rapidly converting the freely determined blood into a nutritive fluid. Let us also call to mind the obstacles afforded to the escape and extraction of the milk in impervious, ill-formed, or diseased nipples, as well as the difficulty that opposes itself to the transmission of milk through ducts, perhaps for the first time; or after a long suspension of their action, called upon to perform the office of conduits for this new and rapidly formed secretion. These considerations tend to explain a fact observed in practice, that with first children, or after long intervals from child-bearing, more trouble is experienced with the breasts on delivery. When, in addition to what has preceded, we recollect the varying calibre of the conduits, their ramifications, and the accidental obstacles occurring in them, the effect either of present or previous disease narrowing them, or perhaps rendering them altogether impervious; and amongst the most common of these, determination to certain parts of the organ, from exposure to cold, we need not wonder at trouble being so frequently caused by the breasts, after delivery, but rather feel astonished that milk abscess is so rare an occurrence. The rationale of the plan of treatment necessary to adopt will appear, from what has preceded, to be, simply, checking or

preventing too rapid a determination to the breasts, and relieving the ducts and tubes by facilitating the transit and escape of the milk when secreted. In effecting the first object, free saline purgation is necessary in the commencement, after which nauseating doses of tartar emetic appear to act almost as a specific in preventing inordinate determination. They also, no doubt, act in producing absorption. Tartar emetic, however, appears to effect the second object as well as the first, by its well-known power of relaxing contractile tissues, thus rendering pervious the lactiferous ducts and tubes, and accomplishing the double object specified. It is not to be understood, that tartar emetic has been had recourse to in all cases where the breasts were hard and full after delivery, or even that this medicine would prove sufficient of itself to correct their obstructed state. The general plan was to administer saline aperients, rub the breasts most assiduously, foment them, extract the milk from them, if necessary, and improve the state of the nipples, if they were diseased or indented. In the great proportion of cases, this is quite sufficient to produce softening of the breasts with free discharge of the milk. It is only where this plan does not succeed, or that symptoms are urgent, that it is deemed necessary to nauseate the patient. The result of this practice is, that in the Lying-in Hospital, where it is strictly enforced, a case of abscess in the breast is scarcely ever met with. It is but right to state, that in private I have not found the practice succeed so well; a circumstance ascribable simply to the impossibility of having the necessary directions strictly and effectually complied with. Ladies get fatigued with continued friction, and they do not like the nauseating medicine. Where it has been fairly tried, however, its results have been the same as in hospital. Friction, to be attended with benefit, must scarcely for an hour be desisted from, until the hardness disappears and the ducts accommodate themselves to the secretions. The usual orders to our nurses are, "to rub until the breast softens under the hand." Warm oil is used to prevent irritation to the integument from the friction. In inflammation attacking the breasts after delivery, either the glandular or tubal structure, the investing sheath, or the connecting or surrounding cellular tissue may be engaged in the inflammatory action from the commencement, or the inflammation may occur consecutive upon the state of the breasts already described. In either case, throbbing pain, generally redness of the surface, with extreme tenderness to the touch, and hardness at one part, will be present; and these symptoms may be ushered in with a rigor, or rigors may occur in their progress. When this attack is observed, the patient is to be put immediately and freely under the influence of the tartar emetic. If purgation has not been previously attended to, a grain of tartar emetic, given in a full dose of black bottle, is a good commencement. This should be speedily followed by the nauseating mixture, and if vomiting should be twice or thrice freely produced at the outset, all the better, provided our patient can bear it. Warm fomentations must be used, and the breast

drawn by a strong healthy child, or by the exhausted bottle; and friction, if unattended with much pain, is to be persisted in. Leeches, under this plan of treatment, are seldom necessary, and their efficacy, when used, appears very questionable. By a steady perseverance in this plan, in cases even the most unpromising, the inflammation has been again and again subdued.

The child was generally applied to the breast early after delivery, at least as soon as milk could be obtained. Ulcerated and fissured nipples, which did not improve under slight stimulating lotions, as Sir A. Cooper's spirit and borax wash, were touched with a solution of nitrate of silver, 10 or 20 grains to the ounce, a plan found particularly serviceable where there were sprouting granulations or excrescences projecting into the fissure, attended with excruciating pain. In obstinate cases of this kind, even touching with solid caustic was necsssary, and often relieved the patient from immense protracted suffering.

Puerperal Mania.—This form, which constitutes so great a proportion of the maniacal cases met with not merely in the hospitals for insane, but in the incurable cells, when treated properly from the commencement of the attack, is a very manageable disease. Were it only for its efficacy in this distressing malady, the accoucheur should esteem tartar emetic as a most invaluable article in his prescription formulary. In a several years' observance of this medicine's application, in hospital and private practice, but two or three cases have been met with which held out against it, when commenced sufficiently early. The moment a patient was observed to exhibit any incoherence after delivery, attended, as it usually is, with rapid pulse and wild expression of the eye, she was placed under its nauseating influence, and retained so for twenty-four or thirty-six hours, or longer if necessary. In almost every case the disease yielded immediately; the real ills produced by the medicine taking the place of the imaginary ones previously occupying her attention. The only treatment in general necessary, in addition to this, being the administration of aperients, and insuring sleep by free opiates, when the patient had been sufficiently subjected to the action of the tartar emetic. Diagnosis in puerperal mania requires attention. The case most likely to be mistaken for it, is hysteria; an affection of no very uncommon occurrence after delivery. An error in diagnosis, bebetween these diseases, is not, however, attended with any very serious inconvenience in the treatment, as I have found from experience the plan here recommended for puerperal mania proves most effectual in curing hysteria; although it is, perhaps, somewhat more violent than the nature of the case demands. It may be right to mention that two fatal cases of peritonitis occurred in hospital, which commenced with all the symptoms of puerperal mania, and which were treated at first with tartar emetic. In one case, in the course of a few hours from the exhibition of the maniacal symptoms, in the other, after a lapse of two days from their com-

mencement, unequivocal evidences of peritonitis set in, and in both the disease ran rapidly to its fatal termination. A peritonitic tendency was very prevalent at the time. In puerperal mania peculiar care is necessary to prevent the depressing effects of tartar emetic being produced, and with this view a larger quantity of laudanum may be added to the nauseating mixture. Patients labouring under this disease, are not able to bear the same active treatment as some of the other cases alluded to where this medicine has proved efficacious. The pulse in puerperal mania, whilst it is very rapid, is generally small and hard, often wiry, and here a medicine possessing such depressing powers must be used with the greatest caution. A case occurred to me in consultation with Dr. Harvey, of this city, some years since, strongly illustrating the necessity of caution in this respect. We had placed our patient under the influence of the medicine, with the usual directions to her friends of administering or withholding it, as might be necessary. These however were unattended to, and on our next visit we found her in a most alarming state; the medicine having reduced her to such a degree of debility as to deprive her of the power of motion, her pulse was barely perceptible, and extremities and surface quite cold. Stimulants were freely administered, and friction and warm applications used to the surface and extremities, and she immediately recovered, not merely from the remedy, but from the original disease.

Physiological and Chemical Researches on the Blood of the Vena Porta.—The first No. of the forty-fourth volume of *Rust's Magasin* contains an account of some highly interesting researches by Professor Schultz, respecting the chemical and physiological differences between the blood of the vena porta, and that of the arteries and other veins. The following is a succinct summary of the results, as given in the *Gazette Med. de Paris,* (15th August, 1835.)

1. The blood of the vena porta is in general blacker than other venous blood, although this difference is not always manifest to the sight; it is not reddened by the neutral salts, or exposure to the atmosphere, or by the action of oxygen.

2nd. The blood of the vena porta does not generally coagulate, but when it does, the coagula are less firm than those of the other arteries. In those cases in which it has coagulated, it liquifies entirely or partly at the end of from twelve to twenty-four hours, and produces, as well as that which does not coagulate, a black sediment, upon which is formed clear serum.

3rd. The blood of the vena porta contains on an average, when fresh, 5.23 per cent., and when dry, 0.74 per cent. less fibrine than the blood of the arteries and the other veins.

4th. The liquid blood of the vena porta contains generally a little less solid matter (0.18 to 0.3 per cent.) than the arterial blood and the other venous blood.

5th. Its serum contains generally 1.58 less solid matter than

the arterial serum, and 0.80 less than that of other venous blood. In the dry state, the first is of an ash-grey, the second yellow, the third greenish yellow.

6th. The blood of the vena porta contains proportionably more cruor and less albumen; the contrary is the case in the arterial blood; the dry cruor of the vena porta is brownish grey, that of the other veins deep red, that of the arteries bright red.

7th. The blood of the vena porta contains in its solid parts almost twice as much fat as that of the arteries and the other veins. The proportion is as follows:

Blood of the vena porta, - - -	1.66 per cent.
Arterial blood, - - - - -	0.92 „
Venous blood of the other veins, -	0.83 „

8th. The dry serum of the vena porta contains but 0.27 per cent. more fat than the dry serum of the arteries and the other veins.

9th. The albuminous cruor of the vena porta contains 1.11 per cent. more fat than that of the arterial blood, and 1.21 per cent. more than that of the blood of the other veins.

10. It is in the fibrin that this difference is the greatest. The dry fibrin of the vena porta contains 10.70 per cent. of fat; that of the arteries 2.34 per cent., so that the difference is 8.36 per cent.

11th. The fat of the blood of the vena cava is blackish brown and unctuous; that of arterial blood and other venous blood white, or yellowish-white and crystalline; that of the white chyle to two-thirds liquid and one-third crystalline.—*American Journal of Medical Science*, February, 1836.

Eruption caused by the internal Use of Copaiba.—Dr. Thos. T. Hewson, of this city, in a paper in the N. A. Med. and Surg. Journ. v. 72, called the attention of the profession to the occurrence of an eruptive disease resembling rubeola, in persons who had taken the balsam copaiba. The following interesting case of this description of eruption is recorded by Mr. Raleigh in the India Journal of Medical Science, for April, 1834. Mr. H. æt. about 30, was taking for the cure of a virulent gonorrhœa, a mixture composed of balsam copaiba, cubebs, and nitric æther: after a week he felt as if he had taken cold, the eyes were watery and throat a little sore, for two days, when an eruption exactly resembling measles made its appearance on the face, extremities, and body generally; but particularly thickly over the nates and shoulders. The mixture was discontinued, and as he had no febrile symptoms, only a mild laxative was prescribed every morning. After four days the eruption flattened down, and left only copper coloured patches, which in four days more were extinct; no desquamation of cuticle took place; the gonorrhœa continued unabated, and he recommenced copaiba mixture without its occasioning a return of the eruption. He is positive he had measles when a child.

Mr. Raleigh has recorded a second case, in all respects similar to the above, in the same Journal for June, 1834.—*Ibid.*

Dupuytren's Pommade to prevent the Hair from falling out.—Macerate a drachm of powdered cantharides in an ounce of spirits of wine, and filter. Take ten parts of this tincture, and rub it in a mortar, with ninety parts of cold lard.—*Ibid.*

Amenorrhœa.—Dr. Schönlein, late Professor of Medicine at Würzburgh, is of opinion that an injection of aloes, (ten grains in a small quantity of warm water,) thrown up the rectum at the time when the menses ought to make their appearance, is more certain in its effects than any other emmenagogue.—*Ibid.*

Digitalis a Specific for Delirium Tremens.—Dr. Cless, of Wurtenberg states, that he has found digitalis purpurea to be specific in the treatment of delirium tremens. Of thirteen cases of this disease, in which he administered the remedy, all but two recovered; these two had a relapse. The digitalis was given in strong infusion, in doses of a spoonful every two hours. After symptoms of narcotism have made their appearance, recovery ensues.—*Med. Correspond. Blatt. and Gaz. des Hôpitaux.*

Spirits of Turpentine in Enemata as a Cure for Sciatica.—Dr. Ducro, jun., of Marseilles, in an article in La Lancette Francais, (Sept. 15th, 1835,) lauds the efficacy of the spirits of turpentine, administered in enemata for the cure of sciatica. He gives the turpentine in large doses, from one ounce to two ounces and a half, mixed with the yoke of an egg, and vegetable mucilage. He does not state at what intervals the enema is to be repeated; but several seem to have been administered in some cases before relief was obtained.—*Amer. Jour. of Med. Sci.*

Poisoning by Arsenic cured by the Hydrated Tritoxide of Iron.—A remarkable case of this description is recorded in the Gaz. Med. de Paris, (22nd Aug. 1835,) by M. Monod. The subject of it was a hair-dresser, thirty-five years of age, who, in a paroxysm of delirium tremens, swallowed a drachm and a half of white oxide of arsenic. Half an hour afterwards the antidote was given to him, suspended in water, and he drank in twelve hours all the tritoxide produced by the decomposition of five ounces five drachms of the trito-sulphate of iron. He had no violent colic, and twenty-four hours afterwards experienced scarcely any uneasiness.—*Ibid.*

Leucorrhœa cured by Colchicum.—Dr. Ritton recommends the following pills for the cure of leucorrhœa:—℞. Pulv. colchici autumn. gr. iij.; sapon. med. q. s. fit pilul. Three of these are to be taken daily, and the dose increased to five or six. The mean term of cure is ten days. During the treatment, the patient must abstain from fermented and alcoholic drinks.—*Jour. Conn. Med.*

Chirurg., Aug. 1835, from *Gazetta Eclettica de Vérona*, April, 1835.

Soda a Remedy for Tooth-ach.—J. S. Gaskoin, esq. in a communication to the London Med. Gaz. (7th Feb. 1835,) states that tooth-ach may be effectually relieved for many hours by carefully filling the cavity of the decayed tooth with the powdered carbonate of soda. It does not seem to lose its efficacy by frequency of application.—*Amer. Jour. of Med. Sci.*

Pommade for the Cure of Itch.—Dr. Emery, in an article on the itch, in the Bulletin Général de Thérapeutique for May last, highly extols the efficacy of the following pommade for the cure of that disease. Take of brown soap one ounce; table salt half an ounce; sulphur half an ounce; alcohol one drachm; vinegar two drachms; chloride of lime half a drachm. One-fourth to be used in friction, morning and evening, to the hands and feet. He states he has cured by it in the Hospital St. Louis more than twelve hundred patients; many of them in four or five days, others in ten twelve, or fourteen days, the mean period of treatment not exceeding eight days. It has the advantage of never causing any accidents, or eruptive diseases; of not soiling the clothes; of not making the atmosphere of the wards unpleasant; of curing the disease in a short time, and being a very cheap remedy.—*Ibid.*

Pommade for the Cure of enlarged Tonsils.—Dr. Cerchiari, in a communication in the Bull. delle Scienz. Med. de Bologne, May, 1835, extols the efficacy of the following ointment in the cure of enlarged tonsils, caused by repeated attacks of inflammation:—℞ Iodin. pur. ℈j. Ung. Rosar. ℥j. m. To be applied to the tonsils morning and evening, by means of a small brush. By the end of two months these glands will, he asserts, under this application, return to their normal size. It is necessary that the inflammation should be entirely subdued before recourse is had to this ointment. *Journ. de Conn. Med*, Aug. 1835.

Spontaneous Dislocation of the Crystalline Lens.—The India Journal of Medical Science for February, 1835, contains the following interesting case of this rare accident, related by Mr. Raleigh.

"*Dec.* 18*th.*—P. A., a stout sailor, æt. about forty, was placed under my charge, in order to undergo an operation for the removal of a cataract; his own history of his case was, that four or five months since he experienced an attack of redness and pain of this, the left eye, which after a few days' duration left him; from that period his vision had become gradually more and more imperfect, and that during the last two months he had not been able to distinguish any object: he did not remember to have received any blow, either on the eye or head. Observing the lens to be

opaque, and the pupil contracted, I directed the application of belladonna; on the following morning, when proceeding to examine the condition of the cataract, I was surprised to find that the lens had come through the dilated pupil, and now occupied the anterior chamber, and, as its appearance indicated that its circumference was undergoing solution, I allowed it to remain; after about a fortnight, during which time the lens frequently passed from one chamber to the other, it was evident that very considerable decrease in its size had taken place, and as no ill effects were produced on the organ from its present unnatural position, as the man was beginning to distinguish objects, when the lens happened to be in a situation to admit of rays of light passing over its edge, and as the patient's health was robust, I recommended him to allow nature to effect a cure for herself, or at any rate to give her a fair opportunity of doing so,—and discharged him to rejoin his ship, which was on the point of sailing for England."—*Amer. Jour. of Med. Sci.*

On the removal of Sequestra without an Operation.—Dr. Bouget has published a new plan for the removal of sequestra without an operation, in the *Journal de la Societè de Medecine de Bourdeaux,* in an article entitled, " Souvenirs de la Clinique de Delpech."

M. Delpech, discouraged at the unfortunate results in several cases of necrosis of the tibia, turned his attention to measures which might remove the sequestrum without having recourse to the painful operation which is generally necessary. In this search he was successful, for he found that, by means of diluted sulphuric acid, he could destroy the phosphate of lime in the bone to be removed, which is then reduced to its gelatinous parenchyma, and can be easily taken away with the common dressing forceps.

Delpech first employed this application in the year 1814. At this period, the wounded at the battles of Orthes and Toulouse flocked in such numbers to Montpellier, that the Hôpital St. Eloi was soon crowded, and a supplementary one was formed, at the head of which was placed M. C. Fages, since so well known by his valuable lectures on external pathology. Hospital gangrene soon appeared in both hospitals, and caused such extensive ravages that the majority of the amputations terminated fatally; even in those cases which were the most successful, a greater or smaller portion of bone was left exposed by the destruction of the soft parts. A young man, who had undergone amputation of the arm, and had twice suffered from hospital gangrene, which had been with difficulty arrested, had the humerus projecting about an inch and a half beyond the flesh. According to the ordinary treatment the sequestrum would not separate perhaps for months, but it happened far otherwise under M. Delpech's directions. He caused the external surface of the bone to be covered with a pledget of lint, soaked in dilute sulphuric acid, and a wad of the same, equally wetted, to be passed into the medullary canal, whence the

reticular apparatus had been previously removed; at the end of twenty-four hours the portion of denuded bone was so softened that it could be easily detached: ten days after, the extremity of the bone was covered with fleshy granulations, and a complete cure was speedily accomplished.

In the year 1816 a man entered the clinical ward, having a necrosis which extended through the whole length of the tibia. Although he evidently possessed a good constitution, and was apparently capable of undergoing a serious operation, M. Delpech determined to have recourse to the proceeding which had proved successful in the previous instance. He destroyed the soft parts at the upper part of the leg by means of the potassa fusa, and when the eschar, which was about the size of a crown-piece, had sloughed, he applied a pledget of lint, soaked in the dilute sulphuric acid, to the bone; after two or three dressings, renewed every five or six hours, it became soft enough to be taken away by the dressing forceps. This being effected, the application of the potassa, followed by the acid, was made lower down; the sequestrum was exposed to the extent of five or six inches in length, and an inch and a half in width; it was then extracted with the greatest ease. It was more than six inches long, and constituted nearly the two-thirds of a cylinder. The patient left the hospital quite well one month after his admission.

From that time until 1822, when I left Montpellier, adds M. Bouget, I have seen M. Delpech constantly have recourse to this plan of treatment, both at the hospital and in private practice, and always with success. I have also used it myself with advantage in a case of necrosis of the tibia in a child.—*Ibid.*

Elevation and Depression of the Pelvis in Luxations of the Femur.—We give the following observations addressed by M. J. Guerin to the Royal Academy of Medicine:—

"Hitherto it has not been remarked that in luxations upwards and outwards, there exists an elevation of the pelvis at the injured side proportioned to the degree the head of the femur has extended on the external surface of the ilium. The cause of the elevation is, that the superior extremity of the femur, in proceeding upwards on the external surface of the ilium, drags with it the combined tendons of the psoas and iliac muscles, which are inserted into the lesser trochanter. They being reflected on the inferior part of the anterior edge of the ilium as on a pulley, raise the pelvis from their inability to accommodate themselves to the increased distance between their two points of insertion, were the pelvis to remain fixed.

"The consequences of this fact, which can be verified on any pelvis with luxated femur are the following: 1st. In all luxations of the femur upwards and outwards, the shortening of the luxated limb is in a great measure owing to the elevation of the pelvis. 2nd. The pelvis is as much the more raised, the more complete the luxation is. 3rd. In double luxations in old or congenital ones,

the arching of the lumbar vertebræ, and the elevation of the pelvis forwards, are the consequence of the double ascent of the inferior attachments of the psoas of each side on the external surface of the ilium. 4th. In luxations of the femur, not upwards and outwards, the pelvis presents a disposition relative to the new relations which the insertions of the psoas muscles acquire. Thus in luxation forwards or upwards, where the inferior insertion is approached to the superior, the pelvis is drawn upwards at the healthy side, whilst by its depression at the opposite side it apparently effaces or diminishes the shortening of the luxated limb. 5th. In coxalgia, the elevation or depression of the pelvis, which, perhaps, leads one into error as to the lengthening or shortening of the limb, is owing also to the action of these muscles. In the first period of the disease, the pain causing the trunk to incline to the diseased side, relaxes the psoas, and determines the depression of the pelvis at this side, whilst, for the contrary reason, it is drawn up at the opposite side. In the second stage of the disease, where there is luxation or sub-luxation, and oftentimes retraction of the psoas, all causes combine to produce elevation of the pelvis at the diseased side. 6th. There exists after the reduction of the greater number of old luxations of the femur, and even after the cure of coxalgies, although the limbs be perfectly equal in length, and the articular surfaces completely in opposition, there exists, I say, a consecutive halting, owing to the persistent elevation of the pelvis after the reduction of the diseased side. 7th. Finally there exists a halting, either congenital or acquired, in which there is apparent shortening, although the two limbs are exactly the same length, and their articular surfaces perfectly in apposition. This species of halting is not described by authors, and is owing to an elevation of the pelvis at the side which appears shortest."—*Rev. Med.* May, 1836.

Medical Joint Stock Company in Paris.—We fully participate in the feelings expressed by Dr. Oppenheim,* on reading the prospectus of the "*Societé Sanitaire,*" lately advertised in Paris. Like our friend, we were inclined to consider the matter as a mere piece of impudent charlatanism, brought forward for the purpose of puffing, and we felt no inclination to assist the obscure practitioners who were to profit by the scheme, by opposing it, and thus to a certain extent conferring on it the advantage of greater publicity. The *march of events*, to use the Gallic phrase, has, however, revealed some features in this scheme, that necessarily force it upon our notice, for it now appears, that so far from being a hoax, the prospectus was written and planned by persons of no mean reputation, and the purity of whose motives we have no right to suspect. It is enough to mention, as consulting physicians and sur-

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* Zeitschrift für die gesammte Medicin, Hamburg. Band. 2, Heft. 2, p. 258.

geons, and of course as partners in the concern, the names of Marjolin, Lisfranc, Amussat, Jul. Cloquet, Majendie, Biott, Bouillaud, Rostan, Esquirol, &c. &c., to make us pause before we condemn the project as visionary or sordid. The chief conditions of the company are as follow: The members or shareholders are to pay twenty-two francs annually to the treasurer, no matter whether they are well or sick, and in consideration of this annual payment, the company binds itself to provide medical attendance and medicine for all its members when affected with illness. Surgical operations, the management of injuries, fractures, wounds, &c., and even the very teeth of the insured are placed under the especial protection of the company's corps of dentists. To insure attention to its members, the company provide a certain number of practitioners, so many residing in each *arrondissement*, to attend to the members on the sick list, and they appoint a proportionate number of apothecaries, who are bound to make up and dispense the prescriptions of the medical staff; nurses, also, and other minor *aides* are furnished by the company. This scheme may appear to some rational, but to us, we confess, it seems visionary and Utopian, for we cannot bring ourselves to believe that any such company could, by any possible means, fulfil their engagements. If, however, experience proves that the system works well, we shall again recur to the subject; if on the contrary, it does not succeed, or if it is not even carried into execution, (an event the most probable of all, for already have Lisfranc and Rostan withdrawn their names,) we shall of course have nothing further to say about the matter. In the mean time we may observe, that this system, on a smaller scale, has been long prevalent among the trades of Dublin, who, on the payment of a certain weekly sum, are bound to provide maintenance for their members when unemployed, and advice and medicine for them when sick.

From what we know of this system among tradesmen we are not inclined to augur favourably of its workings, especially if tried on a more extensive scale, and we think we see obstacles quite as insuperable to the establishment of joint stock companies, for procuring the aid of doctors, surgeons, and apothecaries, as to the formation of joint stock land companies, joint stock eating companies, or any other similar association. People like to choose their own medical attendants. It is a known fact that mankind value most what costs them most; and they are not inclined to estimate highly either gratuitous or cheap services. How few officers consult the surgeons attached to their regiments, although the latter are, in general, far superior to the fashionable doctor into whose hands our gallant fellow-countrymen, of all ranks and arms, deliver themselves when afflicted with disease. How few students of colleges and universities have recourse to the advice of the medical men who appear in the list of the *ex officio* and salaried appendages of these institutions. With these facts before us, and for many reasons upon which we cannot at present expatiate, we

have no hesitation in declaring ourselves partisans of the *voluntary* system in medicine.

On Catheterism in Paralysis of the Bladder, by Gabriel Stokes, Esq. Licentiate of the Royal College of Surgeons in Ireland, &c. &c.—In all cases of paralysis of the bladder, arising from whatever cause, every practical surgeon is aware of the great importance of emptying this organ of its contents at stated intervals, and of this being effected as perfectly as possible. Now, that the bladder in a state of paralysis cannot be perfectly emptied by the common catheter I am inclined to think; for when a common catheter is passed into a paralyzed bladder, no urine will flow out, but what is on a level with the opening in the back of the instrument, and as the organ cannot contract, none can be raised to that level, but by external pressure, and that pressure never can by any manipulation be made to operate on the bottom of the bladder, so as perfectly to empty it of its contents. Of this I think I had sufficient proof in some cases of typhus fever which occurred to myself; in one of which, although the catheter was introduced regularly twice a day, and large quantities of clear urine drawn off, yet on the patient regaining the contractile power of the bladder, (which he did suddenly,) he passed upwards of 4 oz. of a thick ropy mucus; the only evidence of the existence of which I had before, were a few isolated flocculi in the urine, and in another case, where after drawing off as much as I could in the ordinary way, the patient lying on his back by elevating the pelvis, some ounces more of urine were drawn off.

But it is in cases of paraplegia and hemiplegia, that the greatest necessity exists for the complete emptying of the bladder, and it was a case of this kind which led me to try the effect of an improvement, which occurred to me might be made on the common catheter, and which consists simply in having a second opening at the lowest part of the curve of the instrument. In the case of a gentleman in whom I first tried this instrument, although the common catheter was introduced three times daily by myself, yet in the course of ten days the bladder became extremely irritable, it deposited large quantities of adhesive mucus, it became alkaline, and from having perfect retention, he was affected with incontinence and constant stillicidium, greatly aggravating his suffering and danger. In this state I began to use the catheter with the second opening, but before passing it, I introduced the common one, and drew off a small quantity of urine; when this had ceased to flow, I withdrew the instrument and passed the double-eyed one, and drew off more than an ounce, by measure, of thick mucus. On my next visit I passed the double-eyed catheter at once, and at first there flowed a quantity of thick mucus, after which came the clear urine. In the course of a few days, by persisting in the use of this instrument, the urine became natural in appearance, the stillicidium disappeared, he was able perfectly to retain his urine although still unable to pass it, and all deposition of mucus which was in

enormous quantity, ceased. This patient, who was a highly intelligent medical man, expressed the greatest relief and comfort from having his bladder, as he said, perfectly emptied.

In the case of Edward Gannon, a patient in the Meath Hospital, labouring under paraplegia and retention of urine, after the bladder was apparently evacuated by the ordinary silver catheter, two ounces and a half were withdrawn by means of the double-eyed instrument, and this I did for several successive days; however, in this case, it did not prevent the bladder from becoming irritable.

In a case of retention, arising from enlarged prostate in an old man, Gaghen, a patient in the Meath Hosptial, in which great distention of the bladder had taken place, the double-eyed catheter drew off, after the common instrument had been used, one ounce and a half of urine, and this continued to be observed at each successive introduction of the instrument.

In order perfectly to satisfy myself of the efficacy of this improvement, I performed the following experiment on a *cadaver* recently dead: after having emptied the bladder, I injected into it four ounces of water; with the common catheter I could not by any exertion get off more than two ounces and a half by measure, I then withdrew it, and passed the double-eyed one, and succeeded in getting away one ounce and a quarter.

It may be said, that this small quantity of water remaining in the bladder, will give the patient no uneasiness, and it may not do so, but it does not follow that it will do him no harm, more especially when there is a tendency to the secretion of alkaline urine, as there is in almost all cases of retention. Besides in these cases arising from over distention, nearly the chief indication in their cure is perfectly to empty the bladder at each operation, for if this be not attended to, the great probablity is, that the patient will get an irritable bladder.

With respect to the form of the instrument, the only precaution that occurs to me is, that it should be made strong, for as the opening is made at that part of instrument where force is most applied, it should be made so strong as to preclude the danger of breaking in that situation.

Neuralgia of the Testicle caused by a Calculus in the Urethra.—Mr. C——, aged 52, tall and athletic, sanguineous temperament, sound constitution, having during his life only suffered some slight feverish attacks, an occasional mild catarrh, two trifling gonorrhœas, and a frontal neuralgia, which had immediately yielded to bark, was, while at the theatre and in the enjoyment of perfect health, suddenly seized with a violent pain in the left testicle, rapidly extending to the spermatic cord, then general spasm, extreme agony, and repeated vomiting. This attack lasted fifteen hours, after which it gradually subsided, the pain and other symptoms ceased, and the patient fell into a sound sleep, from which he awoke apparently quite recovered. However, he was soon undeceived, for the next day

but one the paroxysm returned with as great violence as at first, and thus the disease continued, becoming periodic, and assuming a perfect tertian type. The physician in attendance administered sulphate of quinine in large doses, imagining that he had to do with an intermittent neuralgia; to this treatment the disease yielded, and the patient indulged the hope that he was at length perfectly cured; at the end of a month, however the paroxysms re-appeared with as much severity as ever, but not with the same regularity for now there were often intervals of three, four, and even eight and fifteen days between the attacks. Recourse was again had to sulphate of quinine, but it only produced a very slight improvement, and even that was but temporary.

Mr. C—— now went to Paris, and consulted the narrator of the case (M. Reveille Pariset) and M. Marjolin, who after the most careful examination were unable to detect any lesion of the testicle, its appendages, of the spermatic cord, the bladder, or kidnies.

Various remedies were tried, as leeches, baths, opiate enemata, carbonate of iron, extract of aconite, &c., all with equal want of success.

Mr. C——, worn out with pain, disappointed, and tired of unavailing remedies, left Paris. Shortly after a new symptom presented itself, retention of urine, which continued for twenty-four hours, but was perfectly cured by the catheter; fifteen days afterwards the retention occurred again, only lasted three hours, and at its termination, Mr. C—— passed by the urethra a small calculus, the expulsion of which immediately and completely put an end to all his sufferings. This calculus was of an oblong form, pretty much resembling a small elongated olive, its surface smooth, except in two or three places, remarkably hard, about six lines long, and weighing 4,6 grains; from its small size no analysis was attempted. Throughout Mr. C——'s long illness, he never had any symptom of gravel, and his urine was invariably clear and of a natural colour.—*Bulletin Gen. de Therapeutique*, May, 1836.

On the Changes which the Stomachs of Crabs undergo during the period of casting their Shells.—A very accurate account of these changes is given by Dr. K. E. V. Baer, in the sixth number of Müller's Archiv., 1834. Of course it is not consistent with our design to enter into minute details on subjects connected with comparative anatomy; we shall confine ourselves, therefore, to a few points possessed of general physiological interest. Crabs, it is well known, change their shells at a certain season of the year, and it is a very old opinion that they change their stomachs at the same time, a new stomach being formed round the old, which is digested by the recently developed organ. Baer has proved that the crab's stomach consists of two coats, one inner, which in every respect may be compared to a callous, horny epidermis, and which is destitute of vitality, and an outer or containing coat,

transparent, but sufficiently strong and vascular. The inner coat, as is well known, consists of various and very curious parts, some resembling bony plates, others compared to teeth; now at the period when the crab changes its skin, it likewise casts the inner coat of the stomach, and on this account this process, analogous to the moulting of birds, and to the renewing of the hair in quadrupeds, is in the crab attended with very great constitutional disturbance, and a total interruption of the digestive function. Baer relates very accurately the changes which the stomach undergoes preparatory to the casting of its inner coat. It would be beside our present purpose to follow him in this description, however interesting. Some things he mentions are, however, specially worthy of remark; in the first place the softer parts of the old epidermis or inner coat of the stomach are very rapidly digested in the stomach, as soon as it has recovered its functions, and has, which it does quickly, formed a new lining on its inner surface. But there are other harder parts that cannot be so readily digested and dissolved, and which are otherwise disposed of. The hard and hollow bones, popularly termed the teeth, are gotten rid of, being discharged throughout the external orifice corresponding to the mouth. There are other solid plates of the epidermic portion of the stomach, which are not of a shape calculated to irritate the new and tender epidermis, and consequently they can be retained with impunity, and are destined to perform a new and curious function, for according to Baer, these plates, for some time preparatory to the act of casting the shells, rapidly increase in weight and in solidity, so as at the period we are speaking of they may be considered as forming considerable reservoirs of earthy matter, to be gradually dissolved and digested in the newly lined stomach, at the very time when earthy matter is required by the animal for the formation of its new shell. These plates are popularly called *crab-stones*, and when submitted to the digestive process, soon lose their roughness, and become smooth and polished before they are entirely dissolved. These crab-stones are chiefly composed of carbonate of lime, and Baer has proved, by repeated analysis, that the fluid contents of a crab's stomach contain (at the time these stones are in them) a considerable portion of lime, carbonic acid, and muriatic acid. It is interesting to observe, that the chemical investigations of Dulk render it highly probable, that the chief solvent in the crab's stomach is the same acid which plays so important a part in human digestion and in dyspepsia, viz. *free muriatic acid.*

On the Medical Properties of the Tannate of Quinine and Cinchonin.—In Hecker's Annals, December, 1834, is a Report by Dr. Otto of the Transactions of the Swedish Medical Association; the Report contains whatever appeared to Dr. Otto new and important, among the rest the following: Dr. Ronander, the secretary of the association, recommends the tannate of quinine and cinchonin as the most active ingredients of Peruvian bark; he asserts that he has cured by their means several cases of obstinate ague,

which had resisted the use of sulphate of quinine, and other powerful remedies; he has found these preparations also useful in typhus and other states of general weakness, and tendency to putrescence, where sulphate of quinine seemed to be ineffectual. These facts appear to prove, that although *tannin* is not properly speaking a febrifuge remedy, yet it contributes much to increase the efficacy of the febrifuge principles of bark, a conclusion long ago pointed out by Berzelius, who ascertained that the febrifuge powers of different species of cinchona do not depend merely on the quantity of quinine and cinchonin which they contain, but is much influenced by the proportion of tannin that enters into their composition. The tannate of quinine is also a cheaper remedy than the sulphate.

On the Treatment of Obstinate Ague, and of the supervening Dropsy.—The following combination is recommended as very effectual in cases of obstinate old ague, attended with great weakness and torpor of the system, and a tendency to dropsy: ℞ Tannatis Quininæ Piperis nigri Pulv. Extracti Absinthii ā ā ʒss. ℿ Ft. massa in pil. xxx. divide. When all inflammatory and bilious complications have been removed in tertian and quartan agues, three of these pills are to be given every third hour, until two hours before the paroxysm, when they are to be given every hour. In double quotidians and in quartans, the dose must be repeated every second hour. It is sometimes necessary to repeat them continuously throughout two complete intervals, and also during two days of the paroxysms, but then the dose may be diminished. In some it is necessary to substitute a small quantity of opium in place of the pepper. Dr. Collin of Nordköping makes a remark, which has been long verified by British practitioners, that in obstinate agues, which originate in neglected obstructions of internal viscera, particularly the liver or spleen, a short course of calomel forms an excellent preparation for the administration of bark. Dr. Hecker recommends in such cases the combination of quinine and calomel, each a grain. Dr. Collin treats the dropsy, which is so frequently a *sequela* of ague, first with deobstruents such as calomel and conium, and afterwards with tincture of cantharides, commencing with a dose of eight drops, gradually raised to twenty, and repeated three times a day in some mucilaginous medium. He says, that he has never derived any benefit from digitalis in such cases. *Dr. Ronander*, in cases of dropsy preceded by ague and attended with great debility, but not dependant on visceral obstruction, or any evident organic lesion, recommends the following pills: ℞ Sulphatis Cupri gr. x. Aquæ Distillat. q. s. ad solutionem adde Pulveris Radicis Belladonnæ gr. x. Pulv. Zingiberis gr. x. Extracti Absinthii ʒii. ℿ Ft. pil. lxxx. He commences with two pills three times a day, and every second day adds one pill to the dose, until some effect is evidently produced either by the sulphate of copper or by the belladonna. He usually assists the

action of this medicine by the use of mild vegetable diuretic drinks, such as the infusion of juniper berries.

Electricity a Cure for Tænia.—Dr. Sere asserts, that of four cases of tape worm, which he treated by passing electrical shocks through the patient's abdomen, three were cured. The shocks were repeated several times daily, and castor oil was administered at the same time internally. Although most physicians will be inclined to attribute the cure more to castor oil than to the electricity, yet the subject deserves further investigation, as does everything connected with the vitality of the entozoa and parasitical animals. We ourselves had occasion many years ago to make experiments on the Guinea worm, two cases of which were in the Meath Hospital; it was shortly after prussic acid had been introduced as a medicine, and we thought it likely that we could kill the whole of the animal by touching a freshly drawn out and living portion of it with a drop of strong prussic acid; much caution was necessary in order to prevent this acid from spreading along the surface of the worm, so as to reach the edge of its sinuous lodging, for it might thus have proved fatal to the patient. Neither of the worms seemed in the slightest degree affected by repeated applications every morning for four or five days. The Guinea worm has lately become very common at Cairo, and is treated in the usual way by gradual extraction. When the local inflammation and irritation it produces give rise to convulsions and other urgent symptoms, Clot Bey recommends the application of the *actual cautery*.

Anatomical and Physiological Remarks on Hunchbacks.—Although the observations of Dr. Stern, recorded in Müller's Archiv. cannot be said to throw any new light upon this subject, yet they have served to place in nearer juxta-position, and have elicited some curious points of comparison on the organization of this deformed class of persons. Even the most inattentive observer must have remarked the sort of family likeness, both of mind and body, that runs through the individuals labouring under this deformity, a likeness arising not merely from the existence of a hump in all, but from a similarity in complexion, in the general form of the head, and in the care-worn, superannuated appearance of the face. Their limbs, too, have all the same disproportionate appearance, and seem evidently fashioned to serve a trunk of larger proportions. But though the growth of other parts, as, for example, the extremities, has not been suppressed equally with the growth of the trunk, yet neither has the development of these parts proceeded regularly, and it is to this curious phenomenon that the memoir of Stern is directed, for he proves that the different bones of the extremities in hunchbacks do not bear their due proportion to each other. Thus the *thigh bone* is somewhat shorter than it ought to be, even in proportion to the diminished statue of the individual, while the bones of the foot are very large, and suited to a much

taller person; of all the bones, the humerus is proportionally the longest, and to this is owing the great comparative length of the upper extremities in people thus deformed. The skulls of hunchbacks present a very curious proportion between the cranium, properly so called, or brain case, and the skeleton of the face. In fact the former equals in size that of a well grown adult, while the bones of the face remain undeveloped and small, as in childhood; This gives their physiognomy a very curious expression, for in their heads old age and wisdom seem associated with several of the elements of childhood and simplicity. In the form of the lower jaw, in the great size of the mouth, and the compressed flatness of the lips, in the sharp elongated nose, we recognize a striking likeness between all humpbacks. It is curious thus to find that a disease of one, or of a few vertebræ, occurring *at an early period of life,* serves not merely as a foundation for a permanent deformity in the spine itself, but proves the means of modifying the size and shape of even distant organs, such as the bones of the face and of the extremities: an occurrence of this sort taking place before our eyes, and long after birth, teaches us what we may expect from injuries or diseases of any important organ during the growth of the fœtus; prepares us for expecting that certain malformations of central organs necessarily give rise to secondary disturbances in the development of some given parts lying more in the circumference. It would be extremely interesting to determine what influence the *situation of the hump* has in disturbing distant development; is the general formation of the face and limbs, when the hump arises low in the back, different from that which distinguishes those whose humps occupy a situation higher up? It is curious enough, and contrasts strongly with the effects on the facial development produced by a hump on the back, that some infants are born with the face fully formed, but wanting the brain and spinal marrow!

On the Connexion between the Mother and the Fœtus.—Professor Dr. Weber spoke upon the connexion between the mother and the fœtus. In no animal does the blood from the maternal vessels pass into those of the fœtus; but, on the other hand, there is required for the circulating fluids (maternal and fœtal blood) the greatest possible surface of membrane through which their agency upon each other is to be effected. The speaker related three modes of connexion which he had observed. The first is found in all ruminating animals, and in many other quadrupeds. The inner surface of the uterus in these is formed into cellular divisions, resembling velvet, but with cells; into the cells pass certain prolongations of the surface of the ovum with their branches; these cells and the prolongations fitting into them, are both connected by a thick network of very fine vessels, so that the blood circulating in each set is brought into close proximity. The cells and prolongations are but loosely conjoined, and between them is found a chylous fluid, which acts as an intermediate connecting layer; the

second mode of union may be remarked in the carnivora and in many of the rodentia. Here we have a placenta, the uterine and fœtal portions of which are bound together most intimately. The former, which is the most delicate, is separated from the uterus at the birth of the fœtus, and in consequence is soon destroyed.

There is no intermediate fluid; the uterine vessels form a more densely interwoven network with those of the fœtus. As an example of the third mode, we may take the human species, but only conjecturally. Here likewise we find no transmission of blood, but the greatest possible extent of approximated surface. The veins lose their external tunic, and in the placenta possess only their delicate inner coat. The placenta is pierced like a sponge by the transmitted vessels; the arteries and veins from the uterus are intertwined with the tufted vessels of the embryonic portion of the placenta. Whilst the maternal blood circulates in this route the vessels of the fœtal circulation are connected with them as loops and tufts in the interspaces of the spongy mass, and are there as it were washed by the maternal blood. This connexion is the reverse of that in the carnivora, but equally favouring the law of exosmose and endosmose: moreover, the paradoxical proposition of Velpeau, and Breschet, of the adhesion of the chorion to the uterus, by means of a layer of unorganized lymph, is proved to be false by Dr. Weber's exhibition of his analysis of an ovum ten weeks old. It was examined immersed in albumen, which afforded the advantages without the inconveniences of a watery fluid. The society saw that the decidua consists almost entirely of uterine vessels, of coagulable lymph, in which the vessels are formed, and of the tufts of the chorion which are projected between the uterine vessels.—*Translated from Clarus' and Radius' Journal*, p. 366. March, 1835.

Tartar Emetic and Opium in the Delirium of Fever.—(*Note by* Doctor Graves.)—I have continued to derive advantage from this treatment, as described in the two last numbers of the Journal, and have had the satisfaction of receiving most valuable testimony concerning its efficacy from various members of the profession, among the rest from Dr. Prichard of Bristol, a physician distinguished by extensive learning and practical knowledge. The annexed letter from Dr. Green of Youghal was not intended for publication, but it is perhaps on that very account more valuable. I have taken the liberty of printing it, because it corroborates the views I had advanced on a subject, which our readers will at once perceive derives a new interest from the truly instructive paper by Dr. Kennedy on the use of tartar emetic in the diseases of parturient women.

Youghal, 26 June, 1836.

Dear Sir,—In reply to your letter of 21st, I have to regret that my having to remove to and fit up a new residence, together with other matters, so pressed upon my time during the late epi-

demic, that I did not take any notes of cases. I could not hazard a conjecture as to the *modus operandi* of the remedy you mention, but exhibited it in various forms and stages of the fever, and always with advantage. The first case upon which I tried it, was that of a soldier in the military hospital, (of which I had charge in the absence of the surgeon,) who was reported to be dying, had been fourteen days in fever, lying prostrate with muttering delirium, extensive bed sores, evacuations involuntary, total sleeplessness and hiccup. In this case it acted like a charm, the third dose producing quiet, cessation of the hiccup and muttering. I then directed an additional grain of the tartrate to his mixture, and to have it taken every hour; after the first dose of this he fell into a sound sleep which lasted for seven hours, when he awoke, and from that moment steadily recovered. In another case where there was acute bronchitis, I employed it in conjunction with local depletion with a similar result. It appeared to me, after various trials, that this medicine served not only to allay inordinate nervous excitement, but to equalize the circulation in such a manner as to obviate the mischief that might otherwise result from local determinations. On this latter supposition I chiefly tried it, and in every case hitherto (about seventeen) with decided advantage. All of the fever cases alluded to were maculated: in one the subject was a young lady of robust constitution, six months pregnant; the excitement here ran very high, the delirium violent and heat intense. On the ninth day she had been more than forty-eight hours without sleep, when I commenced the use of the mixture in question; it was continued, with various intervals, until the eleventh day, when the delirium, &c. had completely subsided, and rapid recovery took place. It is strange that in none of those cases did it affect the stomach. I found advantage occasionally in altering the proportions of the mixture, and giving it hourly when the excitement ran high. The fever here is now on the decline, and of much milder character. I regret very much that it is not in my power to afford you more information on the subject at present, but hope at some future day I may be able to furnish you with materials bearing on the point in question.

"Yours faithfully,
"R. Green."

Professor Müller *on the Structure of Bone.*—The celebrated John Müller of Berlin published, in 1835, a folio work well worthy of the attention of comparative anatomists and physiologists. It is entitled, *Vergleichende Anatomie der Myxinoiden, der Cyclostomen mit durchbohrten Ganmen;* Comparative Anatomy of the Myxinoid and Cyclostomous Fishes, having perforated Palates.

The first part contains the osteology and myology of these remarkable animals. The second has not yet appeared. This work is distinguished by the extreme accuracy of the anatomical details, and the profound views it contains concerning many points of philosophical and general anatomy; the plates, nine in

number, are highly creditable to Müller's skill as a draughtsman, and to Weber as an engraver.

With the elaborate zoological dissertations, and the laborious anatomical descriptions, of which the greater part of this volume is composed, we have at present no business, but hasten to notice Müller's observations on the structure of bone; a subject to which we have already briefly adverted in a preceding review of a work by Treviranus.

At p. 59, Müller commences a comparison between the skeleton of the myxinoid and that of other cartilaginous fishes, and vertebrated animals. His observations on *the Nature and Structure of the Osseous Tissue in Man*, contain a brief summary of the most recent discoveries made in Germany on this subject, and therefore demand our special attention.

"*Purkinje* and *Deutsch* have lately published some interesting investigations, entitled, *De Penitiori Ossium Structurâ;* diss. inaug. Vratisl. 1834. Tab. i. These investigations have been continued in Berlin by Dr. Miescher, who has for the most part confirmed their accuracy. *Purkinje* and *Deutsch* examined the cartilage of the bone cut into thin plates, and from which the earthy salts had been removed by an acid. Transverse slices of the long bones, when examined by the miscroscope, present cross sections of the longitudinal canals that traverse the substance of the bone; while longitudinal slices exhibited these canals in the direction of their axes. These canals contain marrow, and do not run together very frequently. In bones of a spongy texture they are replaced by cells containing medullary matter. These authors were lucky enough to discover the lamellar structure of the cartilaginous frame work of the bones. In the transverse sections the laminæ presented circular lines described round each of the canals; while in the longitudinal they appeared like lines parallel to the axis of each canal.

"The intervals between the laminæ that surround each minute medullary canal, are occupied by other laminæ that form larger circles round the central medullary cavity of the whole bone; in the broad bones of the cranium, and other flat bones, the laminæ lie parallel to the surface. *Miescher* has entirely confirmed these observations. The microscope discloses also other lines which run across these laminæ, perpendicular to their surface, and which *Deutsch* believed to be the canals that contain the phosphate of lime, a supposition not confirmed by *Miescher*, who found that the first appearance of ossification in the cartilaginous epiphyses of young bones, and in callus, consists in a microscopic dark-coloured network between the corpuscles forming the cartilage. As these lines run across each laminæ, they of course only equal in length the thickness of the latter; now *Purkinje* has made the very interesting discovery, that the cartilage of bone contains many scattered, round corpuscles, of much greater diameter than these lines; Miescher has not only confirmed this discovery, but has

found similar corpuscles in the callus of fractured bones, in the cartilage of unossified skeletons, and also in those portions which either never ossify, or only do so in advanced life; as, for example, in the cartilaginous portions of the ribs and the larynx. These parts consist, as is well known, not of a fibrous, but an homogeneous structure, throughout which these corpuscles are scattered." —p. 59 *et seq.*

Müller remarks upon this subject, that the laminæ visible in the cartilage do not make their appearance until after the formation of the minute medullary canals, and consequently the former cannot be considered as *primitive* constituents of cartilage. He observes, too, that these laminæ distinguish cartilage from periosteum, and serve, along with the presence of the corpuscles above referred to, to distinguish most definitely the structure of the cartilages of bones from that of periosteum, which consists of tendinous fibres. These corpuscles are not the chief seat of the earthy or saline particles, nor does it yet appear how the latter are distributed. When the laminæ of bones, tinged with madder, are examined, the colour appears universally disseminated throughout the whole mass, and consequently as the earthy particles are the cause of fixing the colouring matter, they may be concluded to be equally diffused.

The corpuscles above spoken of, as visible in cartilage after the removal of the osseous particles, by means of an acid, may be distinguished without this in the finest laminæ or plates of the peculiarly delicate cobweb-like osseous reticulation, which we sometimes meet with in the medullary cavity of the human femur. These laminæ are quite transparent, and exhibit the corpuscles of an oval, occasionally rather elongated shape, uneven, and when viewed with a high magnifying power, jagged on the surface. This latter appearance is entirely wanting in the corpuscles of amphibious animals and fishes, and indeed generally is not met with. The outward table of the skull of pigeons exhibits plainly these corpuscles, even before the earthy particles have been dissolved out by an acid.

Quarterly Report of the Carlow Fever Hospital.

Number of patients in hospital the 23rd of April, ...	10
——————— admitted on the 23rd of July, ...	34
Total, ...	44
——————— discharged cured, ...	38
——————— deaths, ...	0
——————— in hospital, ...	6
Total, ...	44

S. Connor, M.D., *Physician to the Hospital.*
A. Jackson, *Secretary.*

Relative Value of Apothecaries' Weights in different Countries.—§ 1. In *England, Germany, Belgium, Sweden, Spain,* the medical *pound* is divided in twelve ounces; the *ounce* into eight drachms; the *drachm* into three scruples; the *scruple* into twenty grains. The ounce, consequently, contains 480, and the pound 5760 grains.

§ 2. In *France,* the medicinal pound contains sixteen ounces; the ounce eight drachms; the drachm seventy-two grains.

In *Rome,* the medicinal pound contains twelve ounces; the ounce twenty-four *denari;* the denaro twenty-four grains.

In *Naples,* the medicinal pound contains twelve ounces; the ounce ten drachms; the drachm three scruples; the scruple twenty *acini.*

§ 3. The following is the relative value of the medicinal pound in different countries, reduced to one standard, the French *gramme,* (equal to 0.06475 of the English grain.)

	Grammes.
England,	373.24*
France,	489.50
Austria,	420
Prussia,	350.78
Saxony, and most of the other German States,	357.56
Bavaria,	360
Belgium and Holland,	375
Sweden,	356.29
Poland,	358.51
Milan,	420
Naples,	320.76
Rome,	339.13
Spain,	230

Annalen der Pharmacie, xii. B. Heft 3.

Professor Müller's *Account of the Arteries that produce Erection of the Penis.*—Professor Müller, of Berlin, has lately discovered, or, at all events, described the arteries of the penis which produce the phenomenon of its erection. We will present the most essential portions of his statements on the subject.

We need not remind our readers that the penis is almost exclusively supplied from the internal pudic artery. This ultimately splits into two branches, the artery of the corpus cavernosum penis, and the dorsal artery of the penis. The former penetrates the corpus cavernosum at its root, and ramifies within it; the latter, after sending twigs to the fibrous envelope of the penis, its fascia, outer skin, and prepuce, terminates by ramifying in the glans.

"The distribution of the principal branch of the arteria penis was all that was hitherto known of this important vessel; it is in the more minute distribution of its branches through the corpus

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* 372-96, Edin. Dispens.

cavernosum penis, that the secret of the cause of erection has remained concealed. It is not a lucky accident, but a systematical arrangement, and investigations proceeding from certain combinations, that have guided me, in this instance, to an important discovery. It is generally thought that the arteria profunda penis, which nourishes the substance of the penis, is at the same time capable of filling the cells of the corpus cavernos. with blood during erection also; that the nutritive blood, as well as that serving for the purpose of erection, proceeds from the fine twigs of the same vessel into the fine (capillary) veins; that it passes inwards from these into the sinuous veins, and again from these to the vena dorsalis penis; and that the state of erection distinguishes itself from the ordinary state of circulation in this organ, partly in the quantity of blood circulating in its vascular net-work, and partly in an impediment offered to the return of the blood through the veins of the penis. The older writers, on the other hand, considered the sinuous veins of the corpora cavernosa to be cells;—imagined that the nutritive blood supplied by the ordinary circulation did not at all reach these cells, or pass through them, but was carried off by appropriate veins; and that still, during the state of erection, the cells of the corpus cavernosum were filled with blood. Both views are, as we shall see, incorrect. The opinion of the old writers is proved to be unfounded, as well by investigations on the dead body as by the vivi-sections of beasts. In many experiments performed on the living horse, dog, ram, &c., I have observed, that upon making an incision in the corpora cavernosa, these bodies were not, in their unexcited state, much charged with blood, but that blood is contained in their sinuous veins, although certainly in less quantity than in the corpus cavernos. urethræ, which bleeds freely when cut across. Farther, in the majority of human subjects, blood is found in the sinuous veins of the corpora cavernosa penis."

M. Müller, it appears, had long thought that erection did not depend on the arrest of the flow of blood in the dorsal veins of the penis. He had also thought that different branches of the artery of the corpus cavernosum served for the purposes of nutrition and erection. It was "one of the happiest days of his life" when he found this, his conjecture, true, and "the wonderful difference" between the two sets of twigs alluded to is described by the Professor as follows:

"I. *The nourishing Twigs* (*Rami nutritii Arteriæ profundæ Penis.*)—When an injection of the arteries of the penis is made with size and vermilion, a considerable portion of the injected mass is always forced into the cavities of the corpora cavernosa, as well in the human organ as in that of the horse and dog. When this mass of injection (of which I am still uncertain as to the means by which it enters into the cellular structure) is washed out, the rami nutritii will become evident. The rami nutritii of the spongy substance (which, since they are upon the walls of the sinuous veins in the interior of the penis, may be also called the

vasa vasorum) are found to be as minute as the arteries of any other part: they distribute themselves upon the pillars of the spongy substance, until they become too fine to be perceived by the naked eye. As in the arteries of other parts, they anastomose; and lastly, they form, as in other parts, the capillary net-work which is so difficult to be injected in the penis, owing to the facility with which the injection escapes into the cavities of the corpora cavernosa.

"II. *Arteriæ Helicinæ Corporis Cavernosi* (*in Man.*)—In order to see these arterial branches satisfactorily, an injection composed of size and vermilion must be thrown into a separated penis, through the art. profunda. (In the horse the pudendal and obturatorial artery are to be injected together.) As before mentioned, a part will escape into the cavities of the corpora cavernosa. When the injection has become cold, the corpora cavernosa must be cut open longitudinally, and that portion of the injection which has escaped into the cells is then to be washed out with great care. If a size of a greater degree of consistence has been employed, it will be found to have become solid on cooling. In this case the penis must be soaked in water, and the mass squeezed out softly and carefully, until the cellular tissue is emptied. When a thin size has been used, this will, of course, not occur; then washing alone will be sufficient.

If the tissue of the corpora cavernosa be now examined with a magnifying lens on its posterior third, it will be seen that, in addition to the distribution of the arteries already described, there is another class of vessels, having an entirely different form, size, and distribution; these branches are short, being about a line in length, and a fifth of a millimetre in diameter; they are given off from the larger branches, as well as from the finest twigs of the artery. Although fine, they are still easily to be recognized with the naked eye; they come off from the artery mostly at a right angle, and projecting into the cavities of the spongy substance, they either terminate abruptly, or else swell out into a club-like process, without again subdividing."

Omitting the references to these vessels in the stallion and the dog, we may proceed with M. Müller's description of them in man.

"These twigs branch off from place to place, sometimes alone, sometimes in greater number: little bundles will be seen, in which from three to ten twigs stand together; these, as well as the former, project constantly into the cells or venous cavities of the corp. cavern. penis. When the arteries thus form a tuft, they arise by a common stem, which immediately divides itself into the separate branches. Sometimes such a vessel, whether it proceeds from the artery as a single branch, or forms part of a cluster, divides itself into two or three parallel branches, which also end either abruptly, or else swell out near their extremity.

"Almost all these arteries have this character, that they are bent

like a horn, so that the end describes a half circle, or somewhat more. When such a branch so divides itself, there are formed doubly bent twigs, inclined one to the other. I have before observed, that many of these arteries enlarge towards their end; this enlargement is gradual, and is greatest at some little distance from the extremity, so that the end is somewhat conical. This cone, however, is rounded at the point, and, giving off no branches, terminates immediately. The diameter of these twigs, in their middle, is from the fifth to the sixth of a millimetre: they preserve a great similarity: thus those which branch off from the large trunk of the artery are not thicker than those which take their origin from the fines subdivisions."

Although these vessels project into the venous cavities, yet they are not entirely naked, but possess a delicate membranous covering.

"The arteries have no openings which can be detected, either on their surface or at their extremities; and if the blood, as it is probable, proceeds from them during erection in greater quantity into the cells of the corp. cavernosa, so it must either pass through invisible openings, or at least through openings which only become enlarged by the great extension of the vessels. If the great number of these tendril-like branches which are given off from the art. profunda penis be considered in comparison with the many fine nutritive twigs of the same vessel, it must be evident that when the former are filled, they must take up by far the greater portion of the blood conveyed by it. The diameter of the art. profunda, therefore, not only includes the nutritive twigs which arise from it, but also the tendril-like branches, which likewise deriving their blood from it, yet, it is probable, allow none to pass except during erection; therefore the blood in the unexcited state of the penis only pervades the nutritive branches, and thus only reaches the commencement of the venous cells in smaller quantities; whereas, during erection, it is probable that the blood passes in quantity through these tendril-formed vessels into the cells."

M. Müller states that these vessels are most numerous in the posterior part of the corpora cavernosa penis, and in the bulb. In the middle and anterior portions of the former, they occur but seldom, and in the anterior part of the corpus spongiosum urethræ they are less frequent; in the glans, M. Müller has not yet distinguished them.

Such is the account of M. Müller. These minute anatomical investigations can be prosecuted by so few, that the statements of a discoverer must be taken for granted, until some industrious injector or dissector arises to confirm or to confute them. As the phenomenon of erection is by no means confined to the posterior part of the corpus cavernosum, it appears rather anomalous that there only these vessels, so essential to that phenomenon in the opinion of our author, should be numerous. This difficulty may, perhaps, be more apparent than real, and we shall congratulate M.

Müller on his discovery, if it is supported by the examinations of others.—*Medical Gazette*, Jan. 9, 1836.

Irish Medical Corporations.

No. 1. *Medical Degrees* (M.B., M.D.,) *granted by the Trinity College, Dublin, during the last Ten Years.*

Years.....	1826	1827	1828	1829	1830	1831	1832	1833	1834	1835	Total
Degrees.....	4	18	16	10	10	17	19	16	19	16	145

Note.—The proportion of M.B. to M.D. degrees may be reckoned, perhaps, as three to one. Very few take the degree of M.D., except such as intend practising in London, and who take an *ad eundem* degree at one of the universities. There was no "commencement" in 1826, which accounts for the small number of graduates.

No. 2. *Admissions into the King and Queen's College of Physicians in Ireland, during Ten Years.*

	1826	1827	1828	1829	1830	1831	1832	1833	1834	1835	Total.
Fellows.....	2	4	2	8	3	1	4	2	0	2	28
Licentiates.....	9	5	8	7	1	5	5	2	0	1	43
Total.....	11	9	10	15	4	6	9	4	0	3	71

No. 3. *Diplomas granted by the College of Surgeons in Ireland, during Ten Years.*

Years.....	1826	1827	1828	1829	1830	1831	1832	1833	1834	1835	Total.
Number of Licentiates.....	14	23	36	29	37	37	27	39	44	29	314

No. 4. *Licenses granted by Apothecaries' Hall in Ireland, during the last Ten Years.*

Years.....	1826	1827	1828	1829	1830	1831	1832	1833	1834	1835	Total.
Licentiates.....	44	87	92	66	52	33	34	55	50	59	572

British and Foreign Medical Review, No. 2.

NOTICES OF WORKS RECEIVED.

"*The Proofs of Infanticide considered; including Dr. Hunter's Tract on Child Murder with Illustrative Notes, and a Summary of the present State of Medico-legal Knowledge on that Subject.* By William Cummin, M.D., Member of the Royal College of Physicians, London," pp. 95, 1836.—This little *brochure* is neither more nor less than what it pretends to be, a summary of the present state of our knowledge on the subject. If the author has advanced nothing positively new, he has stated the case in all its bearings clearly and

concisely. He has reprinted Dr. W. Hunter's little tract, not merely because there is much that is good, but for the purpose of pointing out its errors and deficiencies. It is, in fact, an admirable defence of a female accused of infanticide, inasmuch as it turns entirely upon the possible uncertainty of the various signs of the child's having lived, with a powerful appeal in behalf of the unfortunate female whose life *may* be unjustly endangered. But we quite agree with Dr. Cummin, that the Doctor wrote too much as a partisan, and that he stated fully the objections to the 'tests,' without mentioning the answers to those objections. Dr. C. has appended some excellent notes to Dr. Hunter's pamphlet, with only one of which we are inclined to quarrel. Dr. Hunter stated the objection to the hydrostatic test, that a child will probably breathe as soon as its head is born, and yet may lose its life before the body is born. The true answer to this is, that if the child do actually breathe at this moment so as to inflate the lungs, it will not be destroyed by the delay. Dr. Cummin says, (p. 41,) "now we should say that if this was given as a reason, or urged as a probable ground of the alleged occurrence, it ought to be shown that there was some remarkable deformity of the child's body, or that the navel string was pressed upon so as to cause death by stopping the circulation, (though this was very unlikely, as respiration had begun,) or that strangulation had taken place, the navel string being twisted round the infant's neck, and tightening as the head advanced, &c. We do not conceive any deformity of the child's body necessary to account for a considerable interval between the expulsion of the head and the body; we have often seen it from a total suspension of uterine contraction, and we have heard of children having been lost from this cause alone. Again, as the cord, when twisted round the child's neck, is almost always (in ninety-four out of a 100 cases) much beyond the ordinary length, we do not believe that a child was ever strangled in this way. But it is very seldom that a child breathes in this situation, and as we have said when it does, (so completely as to inflate the lungs, and negative the deductions from the hydrostatic test,) the delay will not cause its death.

After the notes upon Dr. Hunter, we have the author's summary, which is ably executed, especially that part which relates to the hydrostatic test, and the mode of detecting air generated by putrefaction and air introduced by inflation. Dr. C. very justly rejects Daniel's and Bent's tests, and attributes but little value to Ploucquet's, on account of the errors which have been detected by Schandt, Chaussier, and Duvergie, and he might have added by Beck. The evidence drawn from the state of the circulation and condition of lungs will probably enable us to decide whether the child has lived, and Dr. C. has given an accurate *resumè* of the signs of non-respiration and the signs of respiration, which we should be glad to quote did space permit.

He then proceeds to inquire as to the cause of the infant's death, whether it was natural or by violence, and after minute

investigation, these are summed up with considerable ability. We feel great pleasure in recommending this little work to all students of medical jurisprudence, and to all medical men who may be called upon to give evidence on a subject where justice to the community, and humanity to the individual, can only be attained by extensive knowledge and careful investigation.

"*Principles of Pathology and Practice of Physic.* By JOHN MACKINTOSH, M. D., Lecturer on the Practice of Physic in Edinburgh," &c. &c. Fourth Edition, London, 1836.—We are much pleased at the well merited success which has attended the above work, and we most earnestly recommend the present edition to the profession. Many new articles have been added, and all parts of both volumes have been so carefully revised, and so much improved, that we may consider the present edition almost as a new work. It must be peculiarly gratifying to the author to find such ample testimony as he has been able to collect, in the later publications on the diseases of India, concerning the good effects of venesection in the cold stages of ague and other fevers; this practice we have adopted ourselves with great advantage in those cases of ague where an inflammatory affection co-existed with the intermittent.

The article Cholera is well worthy of perusal, and the observations of Dr. Mackintosh on the use of iron in diseases of the spleen, are of great importance, and are borne out by our own experience. We regret very much that the late period at which we received the work in question necessarily prevents us from entering into a more detailed account of its merits. The pathology of the brain and nervous system is treated in a most masterly manner. Again we beg most emphatically to recommend this work to the notice of our readers.

THE

DUBLIN JOURNAL

OF

MEDICAL SCIENCE,

1 NOVEMBER, 1836.

PART I.

ORIGINAL COMMUNICATIONS.

ART. X.—*On the Mechanism of Bruit de Soufflet.* By D. J. CORRIGAN, M.D., Physician to the Charitable Infirmary, Jervis-street, Lecturer on the Theory and Practice of Medicine at the Dublin School of Anatomy, Surgery, and Medicine, &c. &c.

"When no other supposition will account for the appearances, the hypothesis is no longer gratuitous; and it constantly happens, that an inference drawn from an imperfect induction, and which would be on that state of the facts, unauthorized because equivocal, and not the only supposition on which the facts could be explained, becomes legitimate on a further induction, whereby we shew that though the facts first observed might be explained by some other supposition, yet those facts newly observed could to no other supposition be reconciled."—BROUGHAM."

AMONG the signs which are revealed to us by auscultation, there is not one more frequently occurring than bruit de soufflet, or more interesting whether examined as a mere physiological curiosity, or considered as an important diagnostic sign. Yet notwithstanding its frequency of occurrence and its im-

portance, the greatest difference of opinion as to its mechanism, or the manner in which it is produced, still exists among auscultators.

The removal or the explanation of this difference of opinion is most desirable, for until an accurate notion of its mechanism has been obtained, its real value as a diagnostic sign must often be uncertain.

Laennec supposed bruit de soufflet to be the sound of spasmodic action of the muscular fibres of the heart or arteries, according as it occurred in either, analogous in its nature and its mode of production to the rolling murmuring sound produced by ordinary muscular contractions. This opinion need not now be controverted. It has been either tacitly acknowledged or openly averred by nearly all succeeding cultivators of auscultation, that Laennec's theory is untenable, and it is now I believe as generally admitted, that " bruit de soufflet" is strictly a physical sign, in the sense used by Dr. Williams, namely, a " sign depending on the direct operation of known laws of natural philosophy," as contrasted with " a symptom" which is the result of an alteration in vital action. But although the theory of Laennec has been abandoned, no explanation has hitherto been offered of the mechanism of bruit de soufflet, which is, either accurate or adequate.

This singular sound is heard in cases of narrowing of the auriculo-ventricular openings of the heart, and under peculiar circumstances in cases where there is no narrowing.

It is heard in narrowing of the cardiac orifices, and in permanent patency of these orifices.* It is heard, and loudly, in

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* Vide a paper by the writer " On permanent Patency of the Mouth of the Aorta," in Edinburgh Medical and Surgical Journal, No. III. An. 1832.

† It is stated also that the sound is sometimes *absent* where there has been narrowing of the cardiac orifices. I never saw an instance. Dr. Elliotson saw but one, and he does not mention the orifice affected. The existence of narrowing of any of the cardiac orifices without bruit de soufflet, is at all events so extremely rare, that its occurrence must be viewed as an exception to a general rule, so constantly is bruit de soufflet present in cases of narrowing.

varicose aneurism, when an artery of moderate size communicates directly with the cavity of a vein. (Vid. Case of Wound of Brachial Artery and Vein, in Repertoire d'Anatomie; case is related by Dupuytren.) It is heard in the uterine vessels from an early period of pregnancy, and continues as regular as the pulse of the mother until parturition occurs. It is heard in the heart and large arteries of animals dying of hæmorrhage, and frequently for a considerable time in the arterial trunks of persons who have been largely bled. As a temporary occurrence, it may be heard occasionally in the heart of persons in whom this organ is perfectly sound; and lastly, it can be produced at pleasure in any large arterial trunk, as the carotid or femoral artery, by pressing upon it to a certain degree, with the finger or edge of the stethoscope.

Heard under so many and such apparently contradictory circumstances, it is not a matter of surprise, that various and conflicting theories should be entertained as to the mechanism of this sound. It is obvious that no theory of its mechanism can be admitted as correct or sufficient, unless it explains satisfactorily the production of the sound in all instances. It should accord with, and account for, the generation of the sound in all the varied and seemingly opposite circumstances under which the sound is heard; every theory not bearing such test must be rejected, and another sought for, until we arrive at some explanation, "when no other supposition will account for the appearances." The hypothesis, at first gratuitous, will then become a legitimate theory. This mode of investigation has been already attended with good results in other scientific investigations, and it can, I believe, be strictly and correctly applied to the investigation now before us.

Laennec's theory of the production of bruit de soufflet is no longer, I believe entertained by almost any one, and it would therefore be waste of time to advert to it.*

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* For an examination in detail of Laennec's theory of bruit de soufflet, and of

Andral, in the article Auscultation, in the "Dictionnaire de Medecine et de Chirurgie Pratiques," published in 1829, says, that in the present state of our knowledge, no satisfactory explanation can be offered of the mechanism of bruit de soufflet.

In 1824 Bertin's valuable work on diseases of the heart appeared. He says, "nothing can be more easy than to conceive the mechanism of bruit de soufflet accompanying narrowing of the cardiac orifices. The blood forced to pass from the auricles or ventricles must be subjected, in passing through the narrowed opening, to increased friction, and it is this increased friction which produces the sound and fremissement." —p. 226. This theory, tested by the *experimentum crucis* at once falls. It might be admitted as an hypothesis, accounting for the mechanism of bruit de soufflet in the one instance, viz. a narrow cardiac orifice, but it fails when applied to explain the mechanism of bruit de soufflet in other instances.

The sound is heard in permanent patency of the mouth of the aorta, unaccompanied by any contraction of the calibre of the vessel; in the arteries of the uterus during gestation, which are enlarged in their capacities, and in which, on Bertin's own theory, there is *not increased* but *diminished* friction; it is heard in the hearts of animals dying of hæmorrhage, and lastly, it is heard occasionally in the healthy heart and arteries; heard in all these instances without the existence of narrowing to cause increased friction. Bertin's theory, tested by any one of these instances, must, I think, be rejected.

In 1828 Dr. Williams, in his "Rational Exposition of the Physical Signs of Diseases of the Chest," p. 50, suggested that bruit de soufflet and fremissement had their origin in physical causes. He only, however, offered the opinion as a conjecture.

In 1829 the writer of the present paper published in the

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the arguments for and against it, vid. paper by the writer, in the Lancet of 1829, "Inquiry into the Causes of Bruit de Soufflet and Fremissement."

Lancet, an "Inquiry into the Causes of Bruit de Soufflet and Fremissement Cataire." The opinions of that paper shall be noticed a little farther on.

In 1830 Dr. Spittal of Edinburgh, in a very valuable work on auscultation, supposed bruit de soufflet to arise from any cause that *would increase violently* the action of the heart. To this explanation it can be objected, that the sound is often heard in the dying heart when its action is very feeble; again, it is heard in some arteries simultaneously with its absence from others; but if it were dependent on increased action of the heart, as Dr. Spittal supposes, it should be equally loud in all arteries, equally near the heart; and lastly, it can be produced at pleasure in the larger arterial trunks, without at all interfering with or increasing the action of the heart. Dr. Spittal's theory will not account for its presence in the arteries of the pregnant uterus, where it continues to be heard without violent action of the heart; and again, were his theory true, we should always hear it (but we seldom or never do) when there is energetic action of the heart from febrile excitement, or after violent muscular exertion.

In 1832 Dr. Hope, in his work on diseases of the heart, adopted a theory differing very little from Dr. Spittal's. He supposes it to arise from the velocity with which the blood is propelled, &c., velocity alone being sufficient to produce the sound, independent of an increase of *real* force of the heart's contraction.

One of the instances which Dr. Hope adduces in support of his theory, is I think an argument against it; viz. the existence of bruit de soufflet in the enlarged arteries of the uterus. He supposes them relaxed and admitting an increased quantity of blood, and then observes, "the augmented afflux is *necessarily* accompanied with increased velocity of the current." The very contrary must however happen, for in proportion as the arteries of the uterus become enlarged, compared with the

trunk which supplies them, the motion of the blood through them must become slow.*

Were Dr. Hope's theory of increased velocity being the cause of bruit de soufflet correct, the sound should be observed whenever the action of the heart, and consequently the velocity of the blood, is greatly augmented; but it is not: for the sound is almost always absent in high inflammatory fever, and in increased velocity of action produced by exertion. He states that *increase* of *velocity* alone suffices to excite the phenomena, (murmur, and thrill,) independent of an increase of real force of the heart's contraction. If so, it should be always heard in the circulation of the heart of the hectic patient, and in the fœtus, and infants at all times, for in them the circulation is extremely rapid; but it is not heard in the fœtal heart which beats 140 in the minute, while the sound is audible in the circulation of the mother, whose pulse may not at the same moment range at 70, or one-half the velocity.

It is curious that Dr. Elliotson gives as his theory of bruit de soufflet, an explanation of the sound diametrically opposite to Dr. Hope's. (Vid. "On the recent Improvements in the Art of distinguishing the Various Diseases of the Heart," &c., by John Elliotson, M.D., 1830.) Dr. Hope states, that bruit de soufflet arises from increased velocity of motion; Dr. Elliotson says from obstructed motion; he says it is heard in aneurisms, "if the mechanical state of the parts produces

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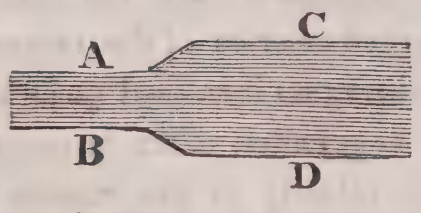
* In accordance with a well known law of hydraulics. Suppose that in a tube



the area of the section C D is double the area of the section A B, then the velocity of the fluid at C D will be only half the velocity of the fluid at A B. If the portion C D, instead of being one tube, be divided into many, the total area and proportion with A B remaining the same, the same rule as to relative velocities will hold true; and if the arteries, as in the placentar portion of the uterus, be large compared with their supply trunk, it is obvious that as they become large, the motion of the fluid through them must become slow.

any difficulty to the course of the blood, or if there be general dilatation of the vessel as well as aneurism, so that the blood is checked when flowing into the narrower healthy portions." p. 35.

Dr. Elliotson says in another part of the same work, (p. 19,) "No disease of the valves, not even a deposit of bone, can be expected to produce it (bruit de soufflet) unless attended by narrowness of the opening." The objections already urged against Bertin's theory apply in all their force to this.

In 1835 Dr. Williams, in the last edition of his work on diseases of the lungs and pleura, says that the conditions necessary to give rise to bruit de soufflet, are to be found "in a certain resistance given to the blood moving with a certain force;" vid. p. 194. His theory appears to be the same as Bertin's, already examined; for he says, "the bellows' murmur produced by pressure on an artery, readily receives its explanation on this principle of resisted motion," vid. p. 195.

Bouillaud in the Archives Generales, (3rd Ann. 11 vol.) says bruit de soufflet may have three causes: 1st, increased moving power in the heart; 2nd, narrowing of any portion of the canal; 3rd, roughness of the surface over which the blood moves. The first of these assigned causes has been already examined under the head of Dr. Spittal's and Hope's theories; the second, narrowing of any portion of the canal, when considering Bertin's and Dr. Elliotson's opinions; the third cause assigned, roughness of surface, may be I think easily disproved; for if it were true that roughness necessarily produced it, it should permanently exist in the healthy heart, for there is in no diseased state a greater irregularity of surface, than there is in the interior of the healthy heart; and it should as invariably be found in false aneurisms, where the internal surface is rough from deposition of fibrine. It is not invariably found in either instance, and therefore roughness of surface has no necessary connexion with the production of bruit de soufflet, and if not necessary, the simultaneous presence of the two is of course but a coincidence.

In 1829 I published a paper entitled An Inquiry into the Causes of Bruit de Soufflet and Fremissement;"* in which it was endeavoured to be proved, that bruit de soufflet is not owing to spasm, (then the doctrine as taught by Laennec,) but is dependent on the manner of the blood flowing; viz. its moving in currents, instead of the equable, progressive movement which is its natural motion.

In that paper I did not bring forward sufficiently prominently the position, that there was requisite also to its production a particular condition at the same time of the parietes of the cavity in which this current-like motion of the blood exists, and without the conjunction of which the sound cannot be generated. The object of this paper is to follow up the theory advanced in the former, to supply its deficiencies, and to endeavour to offer a theory of the mechanism of bruit de soufflet, which may account for its presence in the many varying instances in which it is heard.

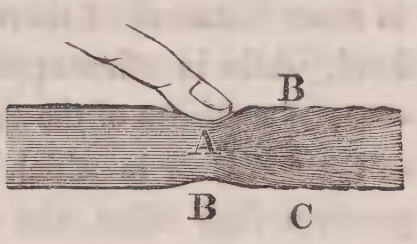
The two conditions which constitute the mechanism of bruit de soufflet are; 1st, a current-like motion of the blood, (instead of its natural equable movement,) tending to produce corresponding vibrations in the sides of the cavities or arteries through which it is moving; and 2nd, a diminished tension of the parietes of the arteries or cavities themselves, in consequence of which, these parietes are easily thrown into vibrations by the irregular currents of the contained fluid, which vibrations cause, on the sense of touch, "fremissement," and on the sense of hearing, "bruit de soufflet." These two conditions I shall endeavour more fully to explain.

If the femoral artery be pressed upon just as it passes under Poupart's ligament, (and a short trial will enable any one to attain the exact degree of pressure,) so as to lessen the calibre of the artery, and thus diminish the supply of blood passing to the portion of the artery below the point of pressure, the con-

* Vid. Lancet for February, 1829.

ditions necessary to produce bruit de soufflet are obtained, namely, a comparatively flaccid state, or a state of diminished tension of the artery beyond the narrowed portion, and a peculiar motion of the blood along this flaccid portion. These two conditions must be explained a little in detail.

In this experiment the artery beyond the point of pressure is no longer tensely distended; for the discharge or waste through the branches being greater than the supply through the narrowed portion, the supply is consequently not sufficient to keep the artery tensely filled; the artery thus becomes in some degree flaccid, and the moment it becomes so, is acted upon by the impulses of the particles of the fluid in motion within. The motion of the fluid within we have now to consider. The stream of fluid issuing from the constricted or narrowed portion of the artery A B as it escapes into the wider portion B C beyond, assumes the form of a current, the particles of which rush along with very different degrees of velocity. The particles in the centre being impelled with the greatest velocity, while those towards the sides are moving with varying and diminished velocities, whirling or currents are thus produced, which impinge upon the sides of the artery, and cause in them corresponding and extremely rapid vibrations: and when the finger is laid upon the artery thus circumstanced, the thrilling vibration of its parietes is perceptible to the touch, and is the "bruissement" of Corvisart, the "fremissement" of Laennec, and upon the ear is made the impression to which we give the name of "bruit de soufflet."* In the ordinary state of the arteries neither of the conditions just described is present. The arteries are always kept tensely distended, and at each action of



* En touchant du doigt l'artere, on reconnaissait que le son etait liè a un leger fremissement de l'artere qui dans ses diastoles semblait venir frotter en vibrant l'extremite du stethoscope.—Laennec, vol. ii. p. 425.

the heart, the additional portion of blood sent forward impels before it the blood already contained in the vessels. The whole body of fluid in the arteries may be in some measure considered as being propelled "en masse" with a nearly equable motion. There is no current-like or rippling motion in the blood itself; there is no thrilling vibration in the parietes of the arterial tubes; the two conditions necessary to generate the sound, are absent; there is no bruit de soufflet, and the motion of the blood is silent.

Other circumstances besides pressure or constriction may give rise to the two conditions described as necessary to cause the sound, and whenever, no matter how produced, these two conditions are present, then "bruit de soufflet" is heard. The necessity of the co-existence of the two conditions explains why in some instances of thoracic aneurism bruit de soufflet is very loud, while in other apparently similar cases it is absent. In cases of aneurism of the thoracic aorta, which involve the mouth of the aorta, or in which the aortic valves do not adequately perform their office, the condition of the parts is remarkably well calculated to produce the sound. In such cases the aortic valves not intercepting the retrograde blood, the sac of the aneurism in the intervals of the heart's action becomes partially flaccid, by throwing back a portion of its contained blood into the ventricle, and at each next contraction of the heart the blood sent in, is whirled into eddies against the sides of a large and flaccid, and often a thin bag, and throws its sides into corresponding thrilling and quick vibrations; and thus in such cases the two conditions requisite to produce the sound are in the highest perfection, namely, a partially flaccid state of the sac or bag of the aneurism, and the impinging upon its sides of whirling eddies of the blood within, and the sound in such instances is, as might be expected, proportionally loud, and is accompanied with strong "fremissement," if the aneurism come within reach of examination by the sense of touch. In other

instances of thoracic aneurism, even of large size and full of fluid blood if there be no derangement of the aortic valves there is no bruit de soufflet; for if the sides of the aneurism be firm, and the semilunar valves not deranged, then the dilated part of the artery is preserved as tensely distended as any other portion of the arterial trunk; there is none of the quick, rippling vibration of the parietes which occurs in the slackened state of the aneurism first described, and hence, there may be observed in the tense aneurism, pulsation in proportion to its size, but no "bruit de soufflet." In short in such instance of aneurism the tensely filled aneurism and the tensely filled arterial trunk are both circumstanced alike. In both there is pulsation, in neither is there morbid sound. This explanation will also account for the occasional presence or absence of bruit de soufflet in other aneurisms besides the thoracic. As long as the sac of an aneurism is in tense apposition with its contained blood there is no morbid sound heard in it; but whenever an aneurism is so circumstanced, whether temporarily or otherwise, that it is not kept tensely distended, then at each new gush of fluid into it there is the rippling motion of the fluid within, and the corresponding thrilling vibration of the parietes of the sac, and bruit de soufflet is heard.*

In narrowing of the auriculo-ventricular opening of the heart, the two conditions necessary to generate the sound are in high perfection, and hence, of all the lesions with which bruit de soufflet is connected, it is in this the sound is most constant. In this disease the loose edge A B of the mitral valve becomes thickened and contracted, so as to make the opening resemble the mouth of a purse almost closed with a running string, while the

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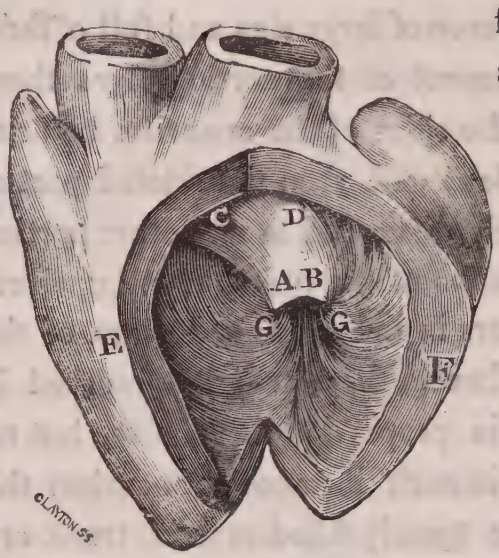
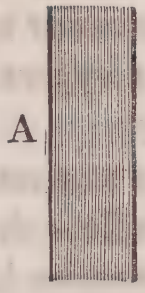
* The principle explained here has been applied in making diagnosis of some forms of aneurism. Vid. paper by the writer "On a new Mode of making an early Diagnosis of Aneurism of the abdominal Aorta," Dublin Journal of Medical Science, vol. ii. p. 375.



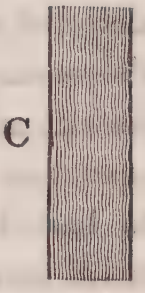
fixed border of the valve C D attached all round to the annulus tendinosus, or margin of connexion between auricle and ventricle, remains unaffected. There is thus formed a perfect funnel, its broad end corresponding with the auricle, its small or discharging orifice projecting into the cavity of the ventricle. The ventricle E F while being filled by the current of blood rushing into it from the auricle, is in the state best adapted for receiving and responding to the impulses or vibrations received from the current rushing into it, for having the instant before, by its own contraction, emptied itself into the aorta, it is (like the flaccid aneurism already described) in a relaxed state; and the current G G being thrown into whirling motions on issuing from the narrow orifice, these whirling or irregular motions throw the comparatively flaccid sides of the ventricle into corresponding vibrations, aud there is thus produced upon the ear during the filling of the ventricle "bruit de soufflet," and if the subject be thin, so that the heart lying close to the parietes of the chest imparts its impulses to them, there is felt by the hand of the observer "fremissement cataire."

In permanent patency of the mouth of the aorta the sound is generally very loud. In this disease the semilunar valves are unfitted for their office, and a portion of the blood is permitted to return back upon the ventricle. Hence the aorta and the large branches, the carotids and subclavians, become in some degree flaccid after each contraction of the ventricle, and they are thus in a state permitting their being thrown into vibrations. The first condition necessary to the mechanism of the sound is present. At each contraction of the ventricle, the portion of blood sent in does not move along a tensely filled aorta,

and in which its motion would necessarily be equable, and attended with little or no sound; but being driven into an aorta, the sides of which are in some degree relaxed, it rushes along (as fluid always will when so circumstanced) as an irregular current. Thus the second condition, namely, an irregular or current-like motion of the blood, is also present. The irregular current within, whirling as it passes along the flaccid tubes, throws the sides of the aorta into corresponding vibrations, and over the sternum corresponding with the aorta there is heard in this disease very loud bruit de soufflet; and in the carotids and subclavians, where they can be examined by the finger, the irregular current-like motion of the fluid within, throwing their tunics into corresponding vibrations, produces upon the sense of touch, the sensation to which we give the name of "fremissement." The diagram A B may be supposed to represent a portion of an ascending aorta, the valves of which being perfect, the vessel is kept fully distended, and the blood within is propelled in a nearly equable motion in tense apposition with the sides of the vessel.



The diagram C D represents a portion of an aorta, in which, from the valves being imperfect, the vessel is not kept fully distended, and the motion of the blood within is, at each impulse from the heart, an irregular rippling rush. In some cases of this disease, there is a distinctly double or see-saw bruit de soufflet; the rushing forward of the blood giving the first or ascending bruit; its retrograde motion or regurgitation into the ventricle through the imperfect valves, giving the second or descending bruit.



I know of no theory of the mechanism of bruit de soufflet, except the one here offered, that will satisfactorily account for the occurrence of the sound in this disease. The theory of increased pressure producing bruit de soufflet will not explain it, for the mouth of the aorta is not narrowed. The theory of

increased velocity will not apply to it, for the sound exists without it, and each individual contraction of the ventricle of the heart in this disease is not more rapid, but indeed generally slower than in health, whether examined by the stethoscope or by the pulse, which is full, and rises without any jerk under the finger. The theory of thickened valves by their vibrations causing the sound is not applicable to it, for the valves are often thinned and wasted instead of being thickened, and in some cases of the diseases the valves remain perfectly sound, the permanent patency arising, not from any morbid state of the valves, but from aneurismal dilatation of the mouth of the aorta, which prevents the valves from closing across its mouth. The presence of bruit de soufflet in narrowing of the mouth of the aorta from irregular growths on the valves, is so easily explicable on the theory now offered, that there is no observation on it necessary.

We now come to one of the most singular instances of bruit de soufflet—that heard in the pregnant uterus. When the uterine region is examined by auscultation at any period from the fifth or sometimes fourth month up to parturition, there is easily detected a loud bruit de soufflet synchronous with the mother's pulse. This sound is generally heard loudest and most distinct and sharp over that part of the uterus to which, on examination after child-birth, it has been found that the placenta had been attached. The two conditions already described as necessary to the mechanism of this sound are found in full perfection in the vessels of the uterus. "The arteries, both spermatic and hypogastric, are very much enlarged. The arterial branches which are most enlarged are those which run towards the placenta, so that wherever the placenta adheres, that part appears evidently to receive by much the greater quantity of blood." Such is Hunter's description. Dr. Evory Kennedy, in his work on Obstetric Auscultation, thus describes the placentar portion of the uterus, on a section being made of it: "The placentar portion, as we may designate it, appears to

consist of a congeries of vessels, tortuous, ramifying, and expanding into cells or sinuses, while the remaining part exhibits the parenchymatous structure of this organ, with merely occasional vessels interspersed." There is, therefore, especially in the placentar portion of the uterus, a stream of blood "rushing," as Dr. Kennedy observes, "from one or two arteries into an extensive system of enlarged tubes." After each contraction of the heart, the pressure of the contents of the uterus from within, and the counteracting pressure of the abdominal muscles from without, empty in some degree or render flaccid the enlarged and tortuous arteries, either by pressing forward into the sinuses the blood which had just been driven into the arteries, or by throwing back a portion of it towards the main trunks, or probably by both. The tortuous arteries of the placentar portion of the uterus, so much enlarged compared with the trunk that supplies them, are, in the intervals between the contraction of the heart, thus brought to the first condition necessary to the mechanism of this sound, namely a comparatively flaccid state, and in this state receive into them the next dash of blood from the supply trunks, and this fresh portion of blood, propelled onwards, runs along in so many rushing currents through the tortuous arteries, throwing the sides of those vessels into corresponding vibrations, and thus giving to the ear bruit de soufflet.

In varicose aneurism, at each diastole of the artery, a jet of blood is thrown into the vein, and this jet at once throws itself, and the more quiescent blood within the vein, into irregular currents, which, playing against the sides of the vein, gives fremissement to the finger and bruit de soufflet to the ear.

There is yet another class of cases which are accompanied by bruit de soufflet and fremissement of exquisite intensity; the cases of those large pulsating tumours which constitute one form of "aneurism by anastomosis," the form described by Breschet in the Memoirs of the Royal Academy of Me-

dicine,* as varix arterialis, (in order to distinguish it from a somewhat similar disease, having its seat in the veins,) and by Mr. Adams, Surgeon to the Richmond Hospital, as the pulsating form of aneurism by anastomosis.† In this singular disease, a tumour presents itself soft and spongy to the feel, pulsating, so as to resemble ordinary aneurism, and when grasped by the hand, felt thrilling with intense fremissement, as if a thousand rapid currents were running in every direction through it. There is also through the tumour every variety of bruit de soufflet, and both the fremissement and bruit de soufflet are synchronous with the dilatation of the large arteries, which are observed running to the tumour. When the structure of such a tumour is examined, it is found to consist of arteries which are immensely enlarged, which are thin and soft, and flaccid in their parietes, and studded thickly with lateral dilatations, twisting and turning upon themselves in a thousand twinings, mixed up with cells, with which they freely communicate, and which cells are often seen to become dark red, and distended at each diastole of the tumour, these cells again freely communicating with veins;‡ the whole tissue of the tumour being actively

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* Memoire sur les Aneurismes, par M. G. Breschet. Memoires de l'Academie Royal de Medicine, vol. iii. 1833.

† Mr. Adams's paper on this disease was read at the meeting of the British Association in Dublin in 1835, and an abstract of it is published in the Transactions of the Medical Section of the Association, in the Dublin Journal of Medical Science for September, 1835. Mr. Adams owes to himself and the profession the publication in full of that paper.

‡ "The coats of the arteries are thin, soft, and flaccid."—"The tumour was found to consist of arteries dilated, flexuous, and forming a web, and the capillary system, arterial and venous, was found greatly dilated."—"The tumour sunk and rose synchronous with the pulsations of the heart. The skin covering the tumour was so thin as to shew a net-work of cells and cavities which the blood at every dilatation coloured dark red. The cells dilated, the skin extended, the volume increased at each contraction of the heart. On making pressure on the common carotid, the tumour became pale and small; and on withdrawing the pressure, the tumour again swelled out in from eight to sixteen pulsations, accompa-

erectile, and resembling, in short, the very structure of the pregnant uterus, in which the same rushing sound of bruit de soufflet is generated.* Such a structure presents, in the highest perfection, the conditions necessary to the mechanism of the sound. The arteries thin, flaccid, dilated, and tortuous, and freely communicating with cells and veins, and partially emptying themselves after each diastole, present their parietes in the state most favourable for corresponding in vibrations with any impulses received from the currents within. The blood sent into those dilated and partially flaccid tubes, whirls in a thousand eddies through them, and rushing along them in currents of every degree of velocity, gives to the thin parietes of the vessels, corresponding thrilling vibrations, and hence the hand laid upon such a tumour receives the sensation of intense "fremissement," while the ear hears the corresponding bruit de soufflet; and if a main trunk supplying the tortuous arteries of the tumour be pressed for a moment, to permit the vessels of the tumour to become more flaccid, then the conditions become still more favourable. On withdrawing the pressure, the tumour again rapidly fills with a loud and strong "bruissement."†

Bruit de soufflet is occasionally heard in the large arteries and heart of persons who have lost large quantities of blood,

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nied with 'bruissement.'"—*Memoire sur les Aneurismes, par* Breschet. *Memoire de l'Academie Royal de Medicine*, vol. iii. 1833. pp. 136-174.

"Aneurism from anastomosis consists in a mutual enlargement of the arteries and veins."—John Bell's *Surgery*, vol. i. p. 456.

* "The tumour is a congeries of active vessels, and the cellular substance through which these vessels are expanded, resembles the cellular part of the penis, the gills of a turkey-cock, or the substance of the placenta, spleen, or womb."—John Bell's *Surgery*, vol. i. p. 457.

† "Pressure being made on the common carotid, the whole tumour became pale and small, and on withdrawing the pressure, the tumour rose again with "bruissement" in from eight to sixteen pulsations."—*Memoire sur les Aneurysmes, par* Breschet. *Memoires de l'Academie Royale*, p. 174.

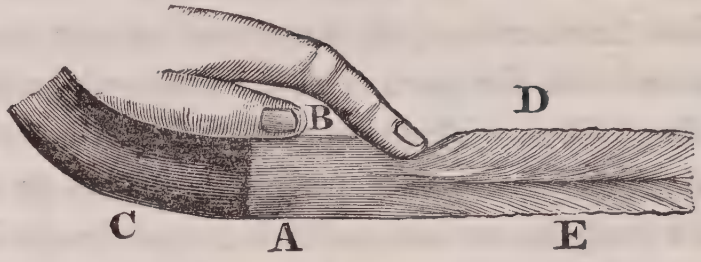
and it is heard, as all who have experimented are well aware, in the heart and arteries of animals bled to large amount. In those instances, from the rapid and copious depletion, the balance of circulation is destroyed, the vascular system is not capable of suddenly adapting itself to the diminished quantity which is now in circulation, and the larger arteries not being in sufficiently tense apposition with the contained fluid, that fluid (the blood) is sent at each contraction of the heart along these tubes, not in a full equable stream, but (as in the instance of flaccid aorta from permanent patency of the aorta) in the irregular motions of a current, through tubes not tensely distended. The two conditions necessary to produce bruit de soufflet are present, and the sound is heard. There is an observation easily made, which confirms this view. When the temporal artery is opened to draw blood from it, if the artery be of tolerable size, and if the incision do not cut the artery quite across, then in addition to what issues from the orifice, a proportion of the blood still continues its course along the artery, rendered evident by the pulsation felt beyond the bleeding orifice. When the finger is placed on the artery beyond the orifice, fremissement is felt, while on the near side of the bleeding orifice, there is the simple pulsation of the vessel. In this instance, the state of the artery beyond the orifice is similar to the state of the large arterial trunks in the instances of excessive depletion. In the temporal artery, a portion of the blood which should pass on, is abstracted or flows out, from the orifice, and the portion of the artery beyond the orifice is not receiving the full quantity of blood required to keep it tense. The small quantity which passes on beyond the orifice no longer moves as an equable stream, but as an irregular current, and the vibrations are felt distinctly by the finger laid upon it, producing fremissement. In this instance, the opening in the side of the artery produces the same effect as pressure in the first experiment related, viz. the

supply of blood is diminished in the one instance from some of it escaping; in the other from its being prevented by pressure from passing on, and the result is the same. In cases of excessive depletion from hæmorrhage, the large arteries near the heart are circumstanced like the portion of the temporal artery beyond the bleeding orifice. They have not their full quantity of distending fluid, and hence they permit the blood to run along them in an irregular current, and this current producing corresponding impulses on their comparatively flaccid sides, the conditions necessary to produce the sound are present, and the sound is heard.

Bruit de soufflet is sometimes heard without any lesion, either of contraction or dilatation, with which to connect it, in hysteric, nervous, or irritable patients. In such persons the action of the heart and arteries is rendered irregular by very slight causes; an instant, although often momentary alteration in the motion of the blood, and a deviation from the natural state of tension of the arteries, is the immediate effect, and bruit de soufflet is heard; but let the cause of agitation, of irregular, hurried, or nervous palpitation be removed, the heart returns to its healthy, regular action, the circulation to its steady, equable flow, the arteries to their natural tension, the sound diminishes, and soon totally ceases. In inflammatory fever there is no bruit de soufflet in the heart or arteries; the heart's action is stronger and quicker than ordinary; the arteries are fuller than natural; the blood is sent forward in equable movement, and there is no sound.

The theory which is here offered of the mechanism of bruit de soufflet, can be illustrated by a very simple experiment. A portion of sheep's intestine, or of leather or gum elastic tube, A B, is attached to the lower end of a block tin pipe, C, the upper end of which is inserted into a vessel of water, which has an elevation of four or five feet. The water is allowed to descend through the metallic pipe C, and along the flexible tube. As long as the flexible tube is kept tense, by a full supply of fluid being allowed to pass along it, the motion of the fluid as at the

portion A B, is like the motion of a fluid in a fully distended artery; it is equable and smooth, and gives out no sound, and



the sides of the tube being kept in tense apposition with the contained fluid, there is no vibrating motion in them, and hence there is no fremissement in the sides of the tube. If pressure be now made by the finger upon any part of the tube, so as to diminish the supply of fluid to the portion of the tube beyond the point of pressure, then the two conditions necessary to constitute the mechanism of bruit de soufflet and fremissement are present, viz., a current-like motion of the fluid within, and a diminished tension of the sides of the tube. The sides of the tube D E become flaccid in proportion as the supply is diminished; the fluid within rushes along in irregular currents, these currents produce corresponding vibrations in the parietes of the tube, and hence over this portion of the tube, as in the experiment on the femoral artery, there is a loud bruit de soufflet, while the sides of the tube give to the finger, by their quick thrilling motion, the sensation which we call fremissement.* Bouillaud in the Journal Hebdomadaire, (3 Ann. vol. ii.) objected to the conclusion drawn from this experiment. He said that it supported his own theory, for that the sound was produced, not in the part of the tube beyond the point of constriction, but in the narrowed point itself, and that it arose from the increased pressure which the fluid underwent in passing through the narrowed portion of the tube.† I have varied the experiment

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* This experiment has been related in the paper in the Lancet already alluded to.

† Bouillaud, however, admits, that in his theory it is quite impossible to account for the production of bruit de soufflet in cases where there is no narrowing.

to obviate his objection ; to the farther end of the flexible tube

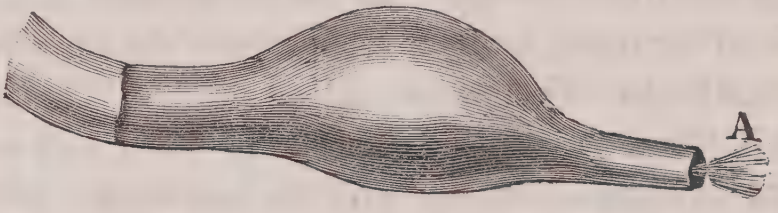
or intestine E F, I attached a pipe A B, with an opening of about half the area of the supply tube, B C, and the fluid was allowed to flow freely through as before. A short period of time elapsed before the intervening intestine became tense, and during this interval, its sides vibrated strongly, even plainly perceptible to the eye, and a loud bruit de soufflet was heard in it, but the moment the intestine became tense, the bruit de soufflet and fremissement suddenly ceased, the fluid all the time continuing to flow freely through. In the experiment, thus varied, there is no narrowing made at any instant of the experiment, so that Bouillaud's objection is void of weight. The bruit de soufflet in this experiment continues to be heard, and ceases to be heard in exact accordance with the law already laid down. It *continues to be heard* as long as the intestine is in a state of diminished tension, or a comparatively flaccid state, which permits its sides to vibrate in accordance with the current-like or irregular motion of the fluid within, and it *ceases to be heard* the moment the intestine becomes so tense that its sides can no longer be thrown into vibrations. To ascertain how far narrowing could of itself tend to produce the sound, a ligature was tied round the central portion of the intestine at G, so as to

diminish its calibre about one-third,* and the fluid allowed to run

* If diminished much more, the portion beyond the ligature could not become tense, and then there would be necessarily a permanent bruit de soufflet.

through as before. The same results were obtained. While the intestine was filling, its sides were in vibrations, and bruit de soufflet was audible, but as soon as the intestine was tensely distended the sound totally ceased. If the narrowing were the *immediate* cause of the sound, and of itself sufficient to produce it, then the sound should have continued as long as the narrowing was kept up. The ceasing of the sound at the instant of full distention, while the narrowing remained unaltered, proves, beyond a doubt, that the narrowing only acts by being a circumstance which is favourable to the production of the conditions on which the production of the sound immediately depends.*

For the intestine was substituted a leather tube, with a bulging on one side, so as to resemble an artery, with aneurismal dilatation, the discharging orifice A being smaller than the



supply tube. The fluid was allowed to flow freely through, discharging itself from the lower orifice. From the difference in size between the orifices, a short interval elapsed before the dilated tube became fully distended, and during this interval, the dilated portion could be felt thrilling under the finger, and from the same part loud bruit de soufflet was heard; but the moment the cavity became tensely filled, then, although the fluid continued to flow through, all sound and fremissement ceased, for the sides of the cavity becoming tensely filled, they could no longer vibrate, and the sound and fremissement ceased. This experiment explains to us the presence of bruit de soufflet

* This experiment also proves that increased velocity is not the cause of the sound; if it were, the sound should persist in the narrow portion, where the velocity of the fluid must be always greater than in the wider portions on either side.

in some aneurisms, and its absence from others. When an aneurism is tensely filled, there can be no vibratory motion in its sides; there is no sound, no fremissement, there is simply, when it comes to the surface, the increased pulsation in proportion to the size; but if from any cause or temporary circumstance, connected either with the sac of the aneurism or vessel entering it, the aneurism can at all empty itself, so as even in a very slight degree to become flaccid, then its sides can vibrate, and the next rush of blood into it, like the fluid in the experiment just detailed, throws these flaccid sides into corresponding vibrations, and there is then, with each new rush of fluid, a bruit de soufflet, and if the aneurism come to the surface within reach of touch, there is "fremissement."*

In using the term flaccid as applied to express the state of the sides of the tubes or cavities which constitute a condition necessary to the production of the sound, I wish merely to express a state of *diminished* tension of these parietes, permitting vibrations in them, as contra-distinguished from a state of the arteries or tubes in which, being tensely filled by a nearly incompressible and inelastic fluid, there can be no vibratory movements whatever in their sides. This, which may be called the vibratory state, can take place in a metallic as well as in a flexible tube, and the production and cessation of sound obey the same laws as in the flexible tubes. The block tin pipe used in the foregoing experiments was beaten in by a blow of a hammer, at about the distance of a foot from its lower orifice; the lower orifice being left of its full size, the fluid was then allowed to flow through it. There was an exceedingly

* It is, perhaps, scarcely necessary to observe, that bruit de soufflet and its varieties, bruit de lime, de rape, &c., and "fremissement," are all considered as having the same origin.

In making these experiments, two precautions are necessary; first, that air be carefully expelled and excluded from the tubes, and secondly, that the intestine or tubes used, be of considerable length, from three to four feet, and with the discharging orifice immersed in a vessel of water, in order to prevent the "bruit" generated at the termination of the tube, from misleading or confusing the ear.

loud and musical bruit de soufflet in the portion of the tube between the narrowed part and the discharging orifice. Its occurrence is readily explained on the principles already laid down. The discharging orifice being left of full size, the fluid passing from the narrowed portion of the tube tended, in rushing towards the discharging orifice, to leave vacuums at the sides; the particles somewhat removed out of the most rapid part of the stream, whirled round to supply these vacuums; the vibratory motion of the fluid in this portion of the tube, communicated its impulses to the sides of the tube, threw them into vibrations, and the sound was the result. No fremissement could be felt, because in this experiment the vibrations are too minute to be detected by the sense of touch. If this explanation be correct, any thing that would stop the vibratory movement of the tube ought to stop the bruit de soufflet, and such is the case. The discharging orifice of the tube was now narrowed to a size less than the constricted part. On permitting the fluid to run down the tube and issue from the discharging orifice, which it did to a distance of several feet with considerable force, no sound whatever was heard in the portion of the tube where before the sound was so loud.* There was no sound now, because, from the discharging orifice being narrowed, the portion of the tube between the constricted part and the discharging orifice was brought to a state similar to that of the intestine when tensely distended, described in the experiment, p. 193; the sides of the tube could no longer vibrate, and there was consequently no sound.

There are, I am fully aware, in attempting to construct a theory of the mechanism of bruit de soufflet, great difficulties arising from our imperfect knowledge of many of the laws which regulate the production of sound, and of the vital actions of the arteries in which this sound is most often heard. I have not dwelt on

* There is in this experiment a trifling sound produced at the discharging orifice, but it is very easily distinguished from the musical bruit, which is produced within the tube.

the latter, because the object of this paper is to explain, as far as I am able, practically, the *immediate* conditions which constitute the mechanism of the sound, and it will, perhaps, be hereafter invariably found, that whenever vital actions in heart or arteries, give rise to the sound, they act not directly, in producing it, but indirectly, by first creating the conditions which constitute its mechanism.*

ART. XI.—*Observations on the Periodicity of Births, shewing the total Number born in each Month; the Number of Premature Children, the Sex, &c. &c.; the Number of Still-born Children, and Children dying; also with regard to the Death of the Mothers, and the most important Complications met with in Delivery, deduced from the Experience of* 16,654 *Cases.* By ROBERT COLLINS, M. D., late Master of the Dublin Lying-in Hospital.

[Read at the Meeting of the British Association, held at Bristol, August, 1836.]

IN laying the present brief communication before the members of this Association, I beg to state, that it is an addition to the numerous tables and calculations in connexion with this subject, which I had the honour of submitting to their notice at a previous meeting. It is taken from a registry kept by me in the Lying-in Hospital of Dublin, with every possible attention to accuracy, and extends over a period of seven years, commencing November, 1826, during which 16,654 births took place. I have little doubt that conclusions drawn from so extensive a record, are calculated to shew with great precision, the objects contemplated, and trust, in this instance they may be found useful, as affording correct information on many points

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* This paper was read at the meeting of the British Association held in Dublin last year, 1835, and an abstract of it is published in the "Notices of Communications to the Association of that year."

hitherto not similarly noticed in any work with which I am acquainted.

The following condensed table exhibits the *total* number of children born in each month; the *sex;* the total *premature* children monthly; *premature first* children; total *first* children monthly; *premature males* in each month, and the number of *first* children *males.*

In thirty-seven of the 16,654 births, the sex of the child from various causes was not noted, therefore they were not included.

Months.	Total Children born *monthly.*	No. of *Males* in each Month.	*Premature* Births in each Month.	Premature *first* Children.	Total *first* Children.	Premature *Males* in each Month.	No. of *first* Children Males.
January,	1493	761	39	18	418	23	209
February,	1315	676	34	10	366	19	190
March,	1475	754	38	15	410	17	216
April,	1382	738	43	12	405	17	225
May,	1375	701	44	14	417	19	203
June,	1352	702	42	10	391	24	216
July,	1389	747	41	17	405	24	221
August,	1366	718	49	14	440	28	234
September	1367	686	34	13	407	16	220
October,	1371	663	54	22	434	26	227
November,	1369	701	34	13	472	19	254
December,	1363	701	46	14	422	23	207
Totals,	16617	8548	498	172	4987	255	2622

Of the 16,617 noted, 8548 were males, and 8069 females; which is nearly in the proportion of *eighteen* males to *seventeen* females.

Four thousand nine hundred and eighty-seven were *first* children;* which is 1 in about $3\frac{1}{3}$. 2622 of the 4987 were males, or 1 in $6\frac{1}{3}$; and 2365 females, or 1 in 7.

Four hundred and *ninety-eight* of the 16,617 children were *premature* births, or 1 in $33\frac{1}{3}$; 293 of the 498 were *still-born*, 231 of which were expelled in a *putrid* state; 255 of the 498 were *males*.

One hundred and *seventy-two* of the 498 premature children were *first* pregnancies, or 1 in $96\frac{2}{3}$. Ninety-six of the 172 were still-born, eighty of which were *putrid;* forty-two of the eighty were males; of the sixteen *not* putrid, seven were males.

Four hundred and *eighty* of the 16,617 were *twin* children; 245 of which were *males;* fifty-four of the 480 were premature births, and 144 were *first* pregnancies. Of the fifty-four *premature*, twelve only were *first* pregnancies.

In order to ascertain the results at *different periods* of the year, with regard to most of the above calculations, I divided the years into *quarters*, as given in the succeeding table:

Quarters.	Total Children born *quarterly.*	No. of *Males.*	*Premature* Births.	Premature *first* Children.	*Total first* Children quarterly.	Premature *Males* quarterly.	No. of *first* Children Males.
Jan. Feb. March,	4283	2191	111	43	1194	59	615
April, May, June,	4109	2141	129	36	1213	60	644
July, Aug. Sept.	4122	2151	124	44	1252	68	675
Oct. Nov. Dec.	4103	2065	134	49	1328	68	688
Totals,	16617	8548	498	172	4987	255	2622

* In this calculation there is a slight variation from that given in my practical treatise, owing to some of the twin-births not having been reckoned.

Thus it appears that a somewhat greater number of births occur in the three *first* months of the year, and the proportion of *premature* births is diminished; yet when we look to the vast number of deliveries, the table exhibits a striking similarity in the separate calculations at these several periods.

In the *still-born* children in like manner, as given below, it is singular how nearly identical the numbers are throughout the year; thus proving that the temperature of the atmosphere little influences these occurrences in our climate. This similarity is not observed in situations exposed to greater extremes, where the proportion of *still-born* children is much increased in the *winter* months; as observed by Quetelet in Flanders, and Caspar in Prussia.

The following table fully proves the correctness of the opinion advanced by Professor Graves in his interesting observations on the subject of still-born children, as to the "mildness of our winter making the difference between the number still-born in the two seasons trifling."*

The total number of children *still-born* in the Dublin Lying-in Hospital during the seven years the medical charge was intrusted to me, was *one thousand one hundred* and *twenty-one*; thus eighty-four occurred in January, and so on:

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
84	74	91	93	85	95	98	97	98	117	83	106

These calculations, containing the result of 16,654 births, and at the same time extending throughout so many years, may safely be considered as affording the best possible information as to the *periodicity* of this occurrence with us.

* Dublin Journal of Medical Science, &c., January, 1836, vol. viii.

The following statement with respect to children *dying* in the hospital, exhibits a similarly near approach at all seasons of the year; thus, of the total number 284, twenty-six died in January, &c.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
26	19	24	25	23	20	25	20	23	30	24	25

The following table shews the periods at which the several *women* died during my residence in the hospital. The total number was 164; of these eighteen died in January, &c.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
18	20	10	13	17	15	6	11	15	11	16	12

Here the *monthly* results are, in some instances, widely different as to the average mortality, compared with the number of births; thus, for example, in February the deaths were twenty, and the births 1315, or in the proportion of one in $65\frac{3}{4}$; and in July the deaths were six, the births 1389, or one in $231\frac{1}{2}$. The variation, where considerable in this table, was caused by the occurrence of puerperal fever; otherwise little or no dissimilarity would have been noticed.

In order to ascertain accurately the result as to *periodicity*, with respect to the most frequent as well as the most important *complications* met with in delivery, I have taken the dates from my registry, and arranged them in tables in the following order:

LABOURS COMPLICATED WITH HÆMORRHAGE OF EVERY VARIETY.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
9	9	12	8	12	10	17	14	9	11	9	11

Here we observe a considerable increase in July, and nearly the same amount in August, which might perhaps be attributable to the elevated temperature of the atmosphere at this season of the year; as the more heated the patient is during labour, the more likely is hæmorrhage to appear. The result, if *fact*, should be a salutary caution as to the necessity of adopting *additional measures* to keep our patient cool in sultry weather and warm climates. Of the 131 cases of hæmorrhage, twenty-four occurred between the sixth month and birth of the child; of these thirteen were accidental and eleven unavoidable.

The following table shews the period of the occurrence of the hæmorrhage.

ACCIDENTAL.									UNAVOIDABLE.								
February.	March.	April.	May.	June.	August.	October.	November.	December.	January.	February.	March.	April.	June.	August.	September.	October.	November.
1	1	1	2	2	2	1	1	2	1	1	1	1	2	1	1	1	2

Sixty-four of the 131 between the birth of the child and expulsion of the placenta; thus five in January, &c.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	e cember.
5	3	7	3	6	3	12	7	6	5	3	4

Forty-three of the 131 subsequent to the expulsion of the placenta.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
3	4	3	3	4	3	5	4	2	4	3	5

LABOURS COMPLICATED WITH CONVULSIONS.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1	2	4	1	2	0	5	1	4	3	2	5

In this attack much irregularity is to be remarked, and that without regard to season; so that even with respect to periodicity, we are unable to discover any trace as to the *cause* of this obscure disease.

LABOURS COMPLICATED WITH RUPTURE OF THE VAGINA OR UTERUS.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
1	2	4	3	2	2	3	3	7	3	3	1

January, December, and September vary considerably from other months in this instance; yet where the cases are not *numerous*, a longer period than seven years would be necessary to arrive at a satisfactory conclusion; nor is this an event likely to be affected by the period of the year.

LABOURS COMPLICATED WITH TWINS.

January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
22	17	21	18	20	24	33	22	18	15	18	12

In these calculations, July and December exhibit the greatest extremes, the former being nearly three times as prolific as the latter; thus indicating *November* to be the most fruitful month in the production of twins, and *April* the least. We also find by the quarterly calculation, that the last is much deficient; the numbers being 60,—62,—73,—45.

Such are the various occurrences, the *periodicity* of which it appeared to me, when taken from so extended a series of observations, might prove interesting; and which, as a record of *facts*, I am desirous to publish.

The most minute particulars, as well as a full detail of the cases on which the tables are based, will be found in my "Practical Treatise on Midwifery," lately published.

ART. XII.—*An Account of a Human Fœtus without Brain Heart, or Lungs: with Observations on the Nature and Cause of the Circulation in such Monsters.* By JOHN HOUSTON, M. D. M. R. I. A., Surgeon to the City of Dublin Hospital, &c. &c.

[Communicated to the Medical Section of the British Association at Bristol.]

A HEALTHY young woman gave birth to twins, between the seventh and eighth month of her first pregnancy. They were born in quick succession, and the placenta came away immediately thereafter. One of the children was, to outward appearance, perfect in every particular, and of the full growth for

its age. The other a female, the subject of this communication, was a monster, and of somewhat smaller size than its companion. Both were alive at the time of delivery, but died almost immediately after. The placentæ were so intimately united as to appear to constitute but a single organ, but the marks of their union showed that one was scarcely half the size of the other. There was a separate cord, and separate set of membranes for each fœtus. The points of attachment of the cords to the placentæ were several inches asunder. The monster was given to me by my friend Surgeon Walton, who was present at the delivery. I regret that the placenta did not accompany it, but I am satisfied that the above description given me of it by Mr. Walton is very correct. In prosecuting the dissection of the fœtus, I had the valuable assistance of my friend Dr. Hart.

The abnormal fœtus had no cranium, and consequently no brain. The spinal column commenced at the sixth cervical vertebra. The two first vertebræ were imperfect, and without spinal canal; all the remainder were perfect in every respect. A large, watery tumour, having somewhat the appearance of a head, occupied the upper part, and extended down along the back. There were no upper extremities; not even clavicles. There were twelve ribs but no sternum. There was no diaphragm, and consequently no subdivision of the cavity into chest and abdomen. There were neither lungs, nor heart, nor thymus gland; and the liver was totally wanting. The kidneys were of inordinate size; nearly filling the abdomen, and pressing upwards, on each side, to the apex of the cavity formed by the ribs: a large ureter arose from the inside of each. No vestiges of supra-renal capsules could be discovered. There was neither stomach nor spleen. The intestine was divided into two parts, the upper, about one inch and a half long, was like a piece of small intestine coiled upon itself, shut at both ends, and filled with mæconium. The lower consisted of about two inches of the ileum with the cæcum and large intestine, naturally formed

and containing abundance of mæconium. There were insulated patches of peritoneum covering the existing viscera, but no uniform, continuous, peritoneal membrane. The organs in the pelvis were all fully and perfectly formed. The bones and muscles of the lower extremities were complete, but these members were much disfigured by œdema: constrictions in the situation of the groins, knees, and ankles, gave to them the appearance of gelatinous bags joined end to end. The soles of the feet and toes being comparatively free from œdema, were the only parts in which the exterior semblances of limbs were traceable.

The umbilical cord of the monster was considerably shorter than that of its companion, and there was not so much of a twisted arrangement of the vessels. A round tumour, of a cheesy consistence, and as large as a small filbert nut, lay in the substance of the cord about one inch outside the navel, the effects of which on the umbilical vessels are highly deserving of our attention in the question respecting the course of the circulation in the monster. The vein was singularly dilated, elongated, and tortuous from the seat of the tumour as far back as the placenta; and the arteries were palpably wider between the tumour and the body of the fœtus, than on the side leading from it to the placenta; but the change in size, the result, no doubt, in both of the presence of the tumour, was more considerable in the vein than the arteries. The vein, having passed through the umbilicus, descended between the abdominal muscles and peritoneum, nearly as far as Poupart's ligament, and there opened into the right external iliac vein in the situation of the epigastric. The iliac vein was enlarged in size from this point, in a proportion equal to the acquisition it had gained, and from it all the veins of the body were derived. Large veins passed to the pelvis and thighs: numerous twigs ran into the kidneys: others proceeded to the intercostal spaces: and the remainder ascended to the textures constituting the tumour in the situation of the head. These veins were all completely devoid of valves:

coarse injection passed, as through arteries, to their extreme branches in the toes, and, even into the fine ramifications among the muscles; and that, too, notwithstanding that several openings existed in the vessels at the time of making the experiment, through which the injection might have escaped, had any resistance from the presence of valves been offered to its easy transmission. The description of the arterial system may be taken up at the termination of that of the venous. Branches arose by fine capillary roots out of every texture in the body. These coalescing, gradually formed larger trunks, which, when united, constituted a sort of aorta or central vessel, like the aorta in fishes, on the forepart of the spine. The arteries from the superior region of the body coalesced in a very indefinite manner—those from the large kidneys being the most striking and numerous. The arteries from the lower extremities were arranged in the usual manner, and joined the aorta on the lumbar vertebræ. The aorta and cava observed their natural relations. The umbilical arteries took origin from the internal iliacs, and ran to the umbilicus, at which point they entered the cord in the usual manner. The left trunk was considerably larger than the right. No communication existed any where between the arteries and veins, except at the capillary terminations of the vessels. There was nothing analogous to the arrangement consisting of ductus venosus, foramen ovale, and ductus arteriosus, designed to connect the two systems of vessels in the perfect fœtus, and to transmit the blood brought from the placenta by the veins into the arterial trunks of the embryo. By whatever system of vessels the blood entered this fœtus, by them it must have been distributed through all the tissues of the body. Such were, in general terms, the conditions of the different organs of this imperfect being. Each might afford a distinct topic for consideration; but I shall confine myself for the present to an inquiry as to the nature and cause of the circulation of the blood.

My observations shall be founded on the state of the circulating organs in the monster which I have described, and on

that in well authenticated cases of the same nature, reported by other physiologists. It is a subject which has been frequently discussed, and about which there still exists considerable difference of opinion.

Monro suggests, that the blood is carried from the placenta to the fœtus by the umbilical vein, and is distributed, by the branches continuous with it, all through the body of the fœtus, and that it is thence returned to the placenta by the arteries which, in that case, like the veins, have their functions in the body of the fœtus reversed. Blandin rejects this hypothesis on account of a supposed valvular organization of the veins in the body of the fœtus: he concurs, however, with Monro, that the blood leaves the placenta by the umbilical vein; but to meet the difficulty respecting the valves in the body of the fœtus, he is obliged to hazard the supposition that the umbilical vein must communicate in some way with the trunk of the aorta. Tiedemann, Sir B. Brodie, and Breschet are of opinion that the blood passes from the placenta to the fœtus by the arteries, and returns to the same organ, in completing the circle, by the vein; in other words that the circulation in the placenta and cord is inverted, whilst that in the body of the fœtus preserves the natural course.

In a paper which I read before the Surgical Society of Ireland in the month of March last, I adduced circumstances in connexion with the case above related, which appeared to me decisive as to the course of the circulation in that case. I arrived at a conclusion similar to that of Monro, that the blood passed from the placenta to the fœtus, and was distributed through its body by the veins, and that it was carried out of the fœtus again and re-distributed on the placenta by the arteries. Since the reading of that paper much interest has been given to the subject by the report of the dissection of a case by Sir A. Cooper, in which he demonstrates the existence of an anastomosis between the vessels of the cord of the monster and those of a perfect twin infant which lay with it in the

womb. The dissection of this case, from the care and judgment with which it was conducted, throws much light on the subject; but I do not consider that the presence of such an anastomosis as that alluded to is a circumstance of a nature to alter my view of the course of the fluids between the placenta and fœtus, nor to justify the conclusions to which Sir Astley has been led by the discovery of it. I was aware, at the time of making my original communication, that a case had been reported by Dr. Clarke, in which injection passed from the cord of the perfect child into both placentæ, and even into the body of the monster—a condition analogous to that described by Sir A. Cooper—and I reported the circumstance as a strong argument in favour of the view which I advocated of the course of the circulation in the monster. Sir Astley considers, "that the imperfect child was a mere appendage to that which was perfect"—"that the imperfect fœtus received through its own umbilical artery, from that of the perfect fœtus, the current which, after entering the aorta and thence circulating through the arteries and veins of the monster, was conveyed back again by the umbilical vein of the imperfect, to that of the perfect fœtus; and that the heart of the developed child impelled the blood into the other which was appended to it, in the same way that it caused that fluid to circulate through the vessels of one of its own limbs." Sir Astley's idea, then, of the direction of the currents to and from the monster, is the same as that held by Tiedemann, Brodie, and Breschet, namely, that the blood enters and is distributed through its body by the arteries, and is thence returned to the placenta by the veins;—the improvement suggested by him being, the discovery of the direct influence of the heart of its companion in utero, as a propelling power to the blood.

Let us examine this hypothesis a little closely, and many objections towards its adoption will present themselves. Sir Astley considers that a part of the venalized blood of the perfect fœtus passes, without entering the placenta, into the

monster, through the branch of communication which he has discovered between the umbilical arteries; and that having been distributed through its body by the aorta, it is brought back by the umbilical vein; which, by means of a branch similar to that connecting the umbilical arteries of the two cords, transmits this same blood directly into the umbilical vein of the normal fœtus to be again circulated, though now in a doubly impure state, through the tissues of that fœtus.

The quantity of blood thus passing to and fro between the two beings, without receiving any renovating influence from the placenta, must be very considerable; it must bear a proportion to the size of the abnormal fœtus, if it be the fact that all the blood of that fœtus is furnished to it in this way. Moreover, the heart of the healthy fœtus should be of inordinate size to accomplish the double duty which it is thus called upon to perform. With such impure blood circulating through its body, and with such a heart as would be necessary for the offices thus assigned to it, it is not likely that such a fœtus should ever arrive at the condition of a perfect child; and nevertheless the contrary is the fact, as attested by Sir Astley Cooper, Dr. Hodgkin, and others. Respecting the twin of one of the monsters described by Sir Astley Cooper, it is said, that "it was a fine, healthy, female child;" and again, that "the living child has been perfectly healthy since birth."

These considerations are, of themselves, calculated to create doubts respecting the truth of the explanation afforded by Sir Astley; but there are others of a more demonstrative tendency against it. In speaking of the umbilical cord of the monster, Sir Astley has stated with great clearness, that "the umbilical artery divided into three considerable branches upon the smaller portion of the placenta to which it was distributed;" and farther, "that the umbilical vein was of large size, and received branches of veins from the smaller portion of the placenta, which corresponded with the branches of the umbilical arteries;" and such, no doubt, is the state of the vessels in every instance in

which the cord of the monster and that of the normal fœtus take attachment at distant points of the placenta, and in which there are separate membranes. But what can be the course of the blood in those "considerable" branches of the abnormal cord which are thus distributed on the smaller placenta, if that which circulates through the main vessels takes the inverted course described by Sir Astley Cooper? Either one or other of two things must occur: either the arterial branches, so distributed, are employed in conveying *into* the smaller placenta some of the blood which has passed from the perfect fœtus through the anastomosing branch, whilst the corresponding veins are engaged in bringing back the same blood and discharging it into the umbilical vein leading towards the monster;—or, the arterial branches convey the blood *out of* the smaller placenta and empty it into the umbilical artery, to be carried in company with that derived from the perfect fœtus, by the anastomosing branch, into the body of the monster; the corresponding venous branches re-distributing a due proportion of the returning blood to the same parts of the smaller placenta. As to the first of these conditions, viz. the passage of the blood into the small placenta by the arteries, and its return therefrom by the veins, it is obvious that such could not happen while the main current in the arteries, as represented by Sir A. Cooper, is flowing from the placenta in the direction of the fœtus, and that in the veins from the fœtus in the direction of the placenta. It is impossible that the branches and trunks of the same vessels, while continuous and open, could transmit the blood contained in them in opposite directions. As regards the second and only other mode by which the blood might be supposed to circulate in the smaller placenta, viz. leave it by the arteries and return by the veins, it is sufficient, in order to render intelligible the impracticability of such a mode of circulation, to state, that this course would be the very reverse of that which naturally and necessarily occurs in the larger placenta. So intimately were these placentæ joined, and so freely did the injec-

tion pass from the one to the other in the case reported by Sir Astley Cooper, that even he has described the whole as one organ; and, as such, all parts of it must have received and have given out the blood in the same manner; for it cannot be accredited that one moiety of it could receive that fluid by the veins and discharge it again by the arteries, whilst the other received it by the arteries and discharged it by the veins. In short, neither of these conditions of circulation can be coexistent with that advocated by Sir A. Cooper, under the circumstances of the cases which he has described. The presence of a second placenta, no matter how imperfect, or how connected with the chief placenta, is incompatible with the direct transmission of the blood from the one fœtus into the other. In order that such a mode of supply could take place the imperfect fœtus should be a mere off-set from the perfect one, the vessels of the chord of the latter being continued into those of the former without any secondary placental connexion whatever—an arrangement which, as far as I can ascertain, has never yet been found to exist; for in all the cases of this nature yet reported, on which reliance can be placed, there have been two sets of membranes and two cords, attached to distant parts of a single or of a double placenta. Such was precisely the arrangement in the case which came into my hands; but whether any anastomosis existed between the vessels of the cords at the placenta, I regret not having been able to determine by experiment; but I have very little doubt that a large portion of the blood of the monster was derived from the placenta of the perfect fœtus by means of anastomoses, such as those which existed in Sir A. Cooper's case, as the placenta from which its vessels were derived was much too small to have furnished a supply of blood proportioned to the size of its body. The condition both of the exterior parts of the body and of the internal organs, and, above all, the disposition of the vessels being exactly alike in the two cases, it cannot be doubted but that the circulation in both was the same.

In addition to the above objections, which naturally arise in the mind against Sir A. Cooper's explanation, several facts connected with my case prove to demonstration that the blood in the vessels of the cord and in those of the body of the monster followed the very opposite course to that suggested by him. The effects of the pressure of the tumour on the vessels of the cord, viz. a dilatation of the vein on the side next the placenta, and of the arteries on the side next the fœtus, decides the point that, in this case, at least, the blood entered the body by the former, and was re-conveyed from it by the latter. The absence of any communication between the arteries and veins in fœtuses without heart, such as that established by the foramen ovale in the perfect state, leads to the difficulty respecting the resistance offered by the valves of the veins to the retrograde movement of the blood; as the entrance of the fluid into the body of the fœtus, under such circumstances, necessarily implies that it shall take this inverted course. This difficulty, however, no longer exists. It has been distinctly stated by Sir Astley Cooper, that "these vessels were well injected from the umbilical vein even to the foot, showing the absence of valvular office." In the case by Dr. Clarke, it is recorded, that "before the internal structure was examined, the navel string of the perfect fœtus was injected, from whence the injection passed readily through both placentæ, viz. that of itself, and that of the monster, and then into the substance of the monster also, as appeared by the redness of the skin." It is not here stated into what vessel the injection was thrown, but there can be little doubt that it was into the vein—the vessel most visible and easy of access, and, that, almost invariably selected for such a purpose: this injection of the umbilical vein of the perfect fœtus, followed not only by the injection of both placentæ, but also by that of the vessels of the monster, even to redness of the skin, may satisfy us that there were no valves in the veins of its body to obstruct the easy progress of the fluid. In the fœtus which came under my own observation, and which had nearly reached

the full period of utero-gestation, there were certainly *no* valves in any of the veins of its body: coarse injection passed, as through arteries, to their extreme branches in the toes, and even into the fine ramifications among the muscles. In fact, in all the cases of this nature, in which the experiment of injection has been tried, the veins have been found destitute of valves: and, why this unusual condition in individuals under such circumstances, and in none others? It is obviously in connexion with the singular peculiarity of absence of heart, or of communication between the venous and arterial systems in the body; and a proof, perhaps, of the inverted course of the blood in their channels. The absence of valves in such cases relieves us, at all events, from the great difficulty of admitting the possibility of inverted circulation in the veins. In reference to the individual which I have described, it may be safely asserted that there was no other course for the blood. This fluid, no doubt, entered the body by the veins, and, seeing that these vessels had no communication with the arteries of the body, except by the capillaries, there can be no question as to its further course: and if such be proved to have been the state of the circulation in one instance, its application to others, of precisely the same description, is rendered easy and intelligible.

I am therefore compelled, by the force of the facts which have been presented to me, notwithstanding the great deference which I pay to every opinion advanced by Sir A. Cooper, to dissent from his proposition in the matter before us. I am of opinion that in monsters of this nature, the placental circulation is normal, viz. that the blood enters the organ by the arteries and returns by the veins equally in the imperfect as in the perfect placenta: that there may be, and I suppose there always is, in such cases, a communication between the vessels of the two, which may account for the diminutive size of the one organ as compared with that of the other; but that the presence of such communication is more in favour of the hypothesis, that

the currents take the same course in both cords, than of that which infers that they run in opposite directions.

As to the circulation in the body of the monster, the facts already stated lead me to conclude, that the course of the blood in the veins and arteries is abnormal, viz. that that fluid enters and is distributed by the veins, and returns by the arteries. The great barrier to our admission of such a course for the blood, viz. the valves of the veins, being removed, the only other difficulty which stands between us and the ready adoption of this theory, is the necessity which it imposes on us of admitting that the veins of the fœtus must have performed the functions of arteries—building up the materials of the body, and producing the several secretions,—whilst the arteries were reduced to the mere subservient office of auxiliaries. Such is, no doubt, a great difficulty, inasmuch as it makes a case in opposition to all our pre-conceived notions regarding the respective operations of these two systems of vessels; but, in our present state of ignorance respecting all vital actions, we must not allow this suppositious difficulty to stand between us and the reception of the numerous facts on which the hypothesis is founded.

Regarding the immediate cause of the movement of the blood in fœtuses devoid of heart, much uncertainty prevails; and the attempts which have been made to explain it, by a reference to the heart of the second fœtus in the womb, only serve to assure us the more of our ignorance of the matter. We have seen that the idea of Sir A. Cooper, that the blood is driven directly from the vessels of the perfect into those of the imperfect being, and back again, is untenable; and there is no other mode by which the remote fœtal heart could operate efficiently. As all the blood of both fœtuses passes through the capillaries of the placentæ, it is obvious that the intervention of that organ between the arteries of the normal fœtus, and the few veins of the monster which can derive origin from them, must considerably neutralize the power of the heart as an agent for the fur-

therance of the circulation in the latter. Nor is it even in accordance with the ideas of physiologists, respecting the influence of the heart on the venous circulation, to suppose that it should perform such an office; for, by the dilatation of its cavities, it is believed to act as a suction-pump on the venous blood of the body; which power, if in operation in the normal twin-fœtus, should exert an attraction on all the blood within the range of its veins, rather than allow any part of it to enter the veins of the other infant over which no such "suction influence" can be exerted. Moreover, it is only on a fractional part of the whole blood passing to the monster—a part proportioned to the capacity of the anastomosing vessel—that the heart could, by possibility, even under the most favourable circumstances, exert any propelling influence—all that portion which circulates exclusively in the smaller placenta being completely out of the reach of its stroke.

Supposing, however, it were even possible that the heart of the perfect fœtus might assist, in a slight degree, to propel the blood into the body of the imperfect one, by what agency then shall that blood be driven out of that body again? It cannot be accredited that the remote heart of the second fœtus would have power, after conducting the circulation in the individual to which it properly belongs, to drive the fractional surplus of its blood, with all the rest belonging to the placenta of the monster, not only into that monster, but out of it again.

It must, therefore, appear evident that the *vis a tergo* imparted by the heart of the perfect twin, cannot be the sole moving cause of the circulation in its abnormal companion, no matter in what light we view the application of the force in question. A heartless fœtus of this kind stands perhaps in the same light, in reference to the influence of a heart on the circulation, as one of the same description without a companion in utero, and in which, of course, the blood should take its rounds, totally unaided by any mechanical *vis a tergo*. But, even in the perfect fœtus, while it remains connected with the womb, the

heart does not fulfil the same important offices as after the individual is brought into a condition of independent life, for there is in all cases a period, and that, too, long after the growth of the fœtus has commenced, in which there is no heart at all, and in which nothing but vessels are discernible; and there are other instances in which, throughout the whole period of fœtal existence, the organ never acquires perfection; and, nevertheless, we do not find that under either of these circumstances the blood fails to flow through the body: whatever parts are developed in such cases receive an abundant supply of that fluid. This question might be still further elucidated by a reference to the state of the circulation in many of the lower animals; but, enough—sufficient has been advanced to shew that the heart is not an indispensable organ in fœtal life, and that the circulation of the blood, at this period, is not solely due to its influence. It grows simultaneously with the other organs, but does not appear so much to have reference to the existence of the fœtus, while in the womb, as that it may be in readiness for operation after birth, when exterior mechanical agencies demand more precision and force of progress in the circulating fluids. Such passive energy on the part of the heart is not without analogy in the body of the fœtus; for there are many other organs, such as the lungs, intestinal canal, &c. which are precisely in this predicament. But in thus questioning the power of the heart, as being solely instrumental in accomplishing the transmission of the fluids through the body of the fœtus, I am not prepared with any new explanation likely to account for the phenomenon. The theory of "vital attraction and repulsions," though conveyed in terms which may be considered more as expressive of the facts than as explanatory of them, appears to me to approximate more nearly to the true one, than any which has been yet broached. And I may here introduce the statement of a fact which appears to have passed unnoticed, but which places in a strong light the influence of vitality on the circulation through animal bodies. The leg of a man amputated by Sur-

geon Trant for mortification after fever, was submitted to me for examination. The foot was black, shrivelled, and dry, and a line of separation had passed deeply through the skin and cellular membrane, a little above the ankle: the foot had been cold and insensible for weeks. I threw some common injection into the tibial arteries, and, to my surprise, this coarse fluid passed into the foot, filling all the digital arteries and even the smaller twigs among the muscles. The other foot, which mortified at the same time, but which was not removed for three weeks subsequently, and until the line of separation had extended into the joint in several places, had exposed all the tendons, and divided the posterior tibial artery completely through, I injected in the same manner, and with the same result. The injection passed along the anterior tibial artery, through all the vessels of the foot, even with more minuteness than in the former instance, and, after entering the anastomosis with the posterior tibial artery, flowed out in a retrograde direction at the inner ankle, from the remote extremity of the aperture produced in that artery by the ulcerative process. Both these preparations I have preserved and placed in the Museum of the Royal College of Surgeons in Dublin. Why did not the blood impelled by the heart of this man flow into all these open vessels with the same readiness as the injection, seeing that there was no coagulum or other mechanical cause to impede its entrance? Why was there no hæmorrhage from the open, though dead, mouth of the posterior tibial artery, when the injection found egress therefrom with so much facility? The answer is obvious: the life of the part being extinguished, the *vital attraction,* by which the blood was induced to enter, had ceased to operate, and that fluid had deserted the vessels: the heart with all its power was not competent to overcome this negative obstacle. And that the heart of this individual had not, by his long illness, suffered any diminution of power may be considered as proved by the occurrence of unusual hæmorrhage during the operation and the recurrence of that hæmorrhage

several times after the stump had been dressed. The heart, therefore, even in the adult human body, is not an all-powerful agent in propelling the blood through the vessels; without the presence of some vital attraction in every part, its power is unavailing.

What the action of the placenta may be, in promoting the circulation in the fœtus, it is difficult to say; but that this organ possesses an inherent power, a vital force drawing in and ejecting again the blood which traverses its vessels, no one who has ever heard the placental soufflet—the noise caused by the rush of fluids to the spot in which it lies—will, for a moment, question. This rush of blood cannot be accounted for by a reference either to the force of the heart of the mother, or of that of the infant: it is independent of both, and due, no doubt, to the vital endowments of the organ itself, and of the uterus to which it is connected. And it is quite possible to understand that, with such vital properties in the placenta, together with others of a like character co-existing in the fœtus, the circulation may be carried on between them, without any other influence whatever. Such would, at all events, be a much more intelligible explanation of the cause of the circulation in monsters without heart than that which refers the phenomenon to the heart of a remote and disconnected fœtus—an explanation which involves many contradictions and difficulties.

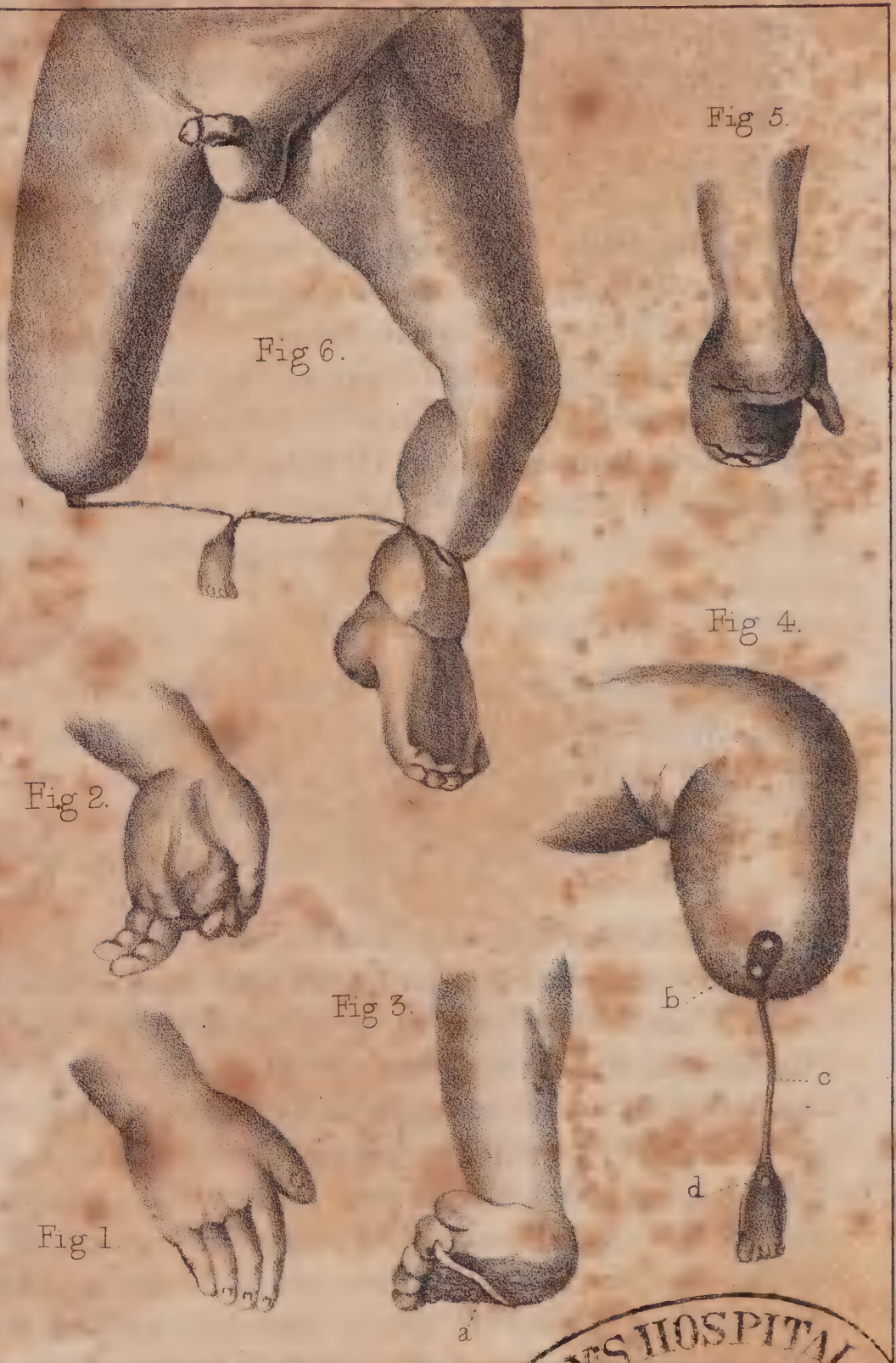
There is still another consideration arising out of this subject, to which I am desirous of directing attention, namely, the possibility that the arrest of development in one of the fœtuses may be the consequence of some unnatural communications between the vessels of the placentæ, such as those discovered by Sir Astley Cooper; and that the normal infant, instead of promoting, by the aid of its heart, the circulation of its feeble companion, may be, in fact, the author of all its imperfections—taking the lead in growth, and, on that account, diverting in its own favour an undue proportion of the nutritive fluids. Every well-attested monstrous production of this nature has been a

twin: in every one, examined with a view to determine the state of the vessels in the placenta, an anastomosis has been discovered between them: and in others, in which no such experiment has been tried, the abnormal placenta has been invariably found too small to have, of itself, furnished a supply of blood equivalent to the bulk of the monster. These facts all indicate a correspondence between a certain condition of placental arrangement and a particular modification of monstrosity in one of the fœtuses. But in what way, or to what extent, such a condition may operate in thwarting the growth, I do not pretend to be able to determine. There appears to me, however, sufficient evidence that the two circumstances stand to each other in the relation of cause and effect, to justify me in hazarding the proposition as a conjecture.

ART. XIII.—*Cases illustrative of the Spontaneous Amputation of the Limbs of the Fœtus in Utero, with Remarks.* By JAMES Y. SIMPSON, M. D., President of the Royal Medical Society of Edinburgh, Corresponding Member of the Society of Medicine of Ghent, &c.

IN the first and second volumes of the Dublin Medical Journal, Dr. Montgomery has offered some interesting observations on the spontaneous amputation of the limbs of the fœtus in utero. He there alludes to Haller, Murat, Richerand, Desormeaux, and Billard, as having mentioned in their writings the occasional occurrence of supposed separations of portions of the fœtal limbs, but refers to three cases seen by Chaussier, (Discours prononcé a l'Hospice de la Maternité 1812, or, Dictionnaire des Sc. Medicales, tom. xvi. p. 70, and tom. xxiv. p. 259,) and one described by Mr. Watkinson, (London Med. and Phys. Journal, vol. liv. p. 38,) as the only accurately detailed cases that he could find on record. To these Dr. Montgomery has himself added two additional instances that had

PLATE I.



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come under his own observation, and which appeared to him to show, that the phenomenon referred to is the result of the constriction of the limb at the point of separation, by a ligature of organized lymph. As the subject forms a very curious question in intra-uterine pathology, I feel assured that I shall be excused for bringing forward the following additional cases;—and the more so on this account, that while they seem to add further evidence in favour of Dr. Montgomery's opinion, they at the same time serve to explain one or two circumstances which the instances recorded by him have left unresolved.

Case I.—In a Thesis by John U. T. Schaeffer, published at Erlangen in 1775, and entitled "Fœtus cum Matre per Nervos Commercium," I find the following example of the spontaneous separation of the fœtal limbs. To the thesis is appended a full-length sketch of the fœtus, from which I have had copied, into the annexed plate, the malformed parts of the extremities. (See Pl. I. Fig. 1, 2, 3, and 4.) A woman, æt. 32, brought forth, in a natural labour at the eighth month, a female child that survived for four hours. The head and trunk were normally formed, but the extremities exhibited the following peculiarities. The thumb of the right hand (Fig. 1) was somewhat thicker than usual, and its nail was wanting; the index finger was naturally formed; but the middle, ring, and little fingers were united together by a common membrane or ligament, which covered them down to their last phalanges, and the little finger had no nail upon it. The thumb of the left hand (Fig. 2) was also destitute of a nail; and its inner surface was united to the metacarpal part of the index finger. Of the index and middle fingers only the third phalanges were present, and these were intimately coalesced with one another. Small fleshy corpuscles supplied the place of the second and third phalanges. The ring and little fingers of this hand were natural. The great toe of the right foot (Fig. 3) was short and somewhat deformed, one of its phalanges appearing to be wanting, and a small but deep cicatrix occupying the situation

in which its nail ought to have been placed. The second toe was also destitute of a nail, and had a kind of membranous cord (a) attached to its extremity. The remaining three toes of this foot were natural; the internal malleolus was reddish, and not covered by cuticle. All the part of the left leg (Fig. 4) below the gastrocnemii seemed, as it were, amputated, and to have the whole surface of the stump covered over with recent cuticle, a small central part only excepted, which was red, and presented the appearance of a recent wound. In this uncicatrized central part (b) the ends of the tibia and fibula were visible; and in the space between them a dense fibrous cord (c) of some length took its origin, having attached to its further or loose extremity a small body, which, on minute examination, proved to be a foot of the size of one belonging to a fœtus of the third month, with divisions on its inferior edge representing the five toes. On the superior part of the foot a minute cartilaginous-like corpuscle was placed, as represented in the figure at (d.)

Case II.—A case in some points resembling that given by Schaeffer, has been published by P. Lagorsky, in the Memoirs of the Imperial Academy of Sciences of St. Petersburgh for 1834. (6th Series, vol. iii. pp. 3—7.) A malformed male fœtus was aborted about the fifth month of utero-gestation, by a woman who had previously borne two well formed and healthy children. The head and face of the fœtus were greatly disfigured. The forehead and superior part of the head were occupied by two large, soft, reddish, oblong-shaped tumours, formed by an expansion and protrusion of the meninges, and containing cerebral substance. The parietal bones, the squamous portions of the temporal, the superior parts of the larger wings of the sphenoid, and the superior part of the occipital bones were wanting, as well as the common integuments of the calvarium. The face was much malformed. The eyes were separated to an unnatural distance from one another, and the left was situated on a higher line than the right. The external parts of the nose were absent, but two foramina leading to the

nasal passages were situated high up on the face. The left cheek was larger and more prominent than the right, owing to its being swelled out by a duplicature of skin arising inferiorly from the neck. The mouth was large, and of a very irregular shape; but the tongue, fauces, and larynx were naturally formed. The helix of the right ear was turned forwards, and united to the skin lying anterior to the meatus auditorius externus. The external parts and internal organs of the chest and abdomen were natural. The prepuce was large, but its orifice small.

Both of the upper extremities were normally formed as far down as the hands, and the right hand was natural in all respects, except in this, that the internal edge of the first or proximal phalanx of the thumb, was intimately united to the corresponding surface of the metacarpal part of the index finger, so that the palm appeared broader, while the thumb seemed shorter than natural, looking as if composed of one phalanx only. The left hand (Fig. 5) was greatly mutilated,—the thumb being present and natural, but the fingers altogether wanting. The inferior margin of the hand was occupied by two little, contiguous, oblongo-roundish bodies, representing imperfect fingers, but formed by productions of the skin alone. In the middle almost of the palm two other transverse, unequally sized bodies were situated, united together at their internal margins, and connected also to another smaller body which rose at the ulnar side of the palm, and had the form of a jointed finger. These three last bodies were, like the two first, formed of skin only.

The leg of the right lower extremity (Fig. 6) was wanting, the thigh ending in a kind of rounded and cicatrized stump, and presenting in the centre of this stump a small acuminated, projecting point. From this apex of the stump was prolonged a slender, thread-like membrane, strong in proportion to its size, that ran nearly transversely across to below the middle of the left leg, which it encircled like a tightened ligature, constrict-

ing it so that a circular depression of considerable depth was formed in the leg at the part, while the portion of the extremity situated below the ligature was, with the appended foot, rather tumefied. From about the middle of this transverse, thread-like membrane an oblong-shaped body was suspended, which, on more minute examination, proved to be the right foot perfectly formed, as its general outline and five toes demonstrated, but not larger in size than the foot of a fœtus of the tenth or twelfth week. Besides, it was removed and dislocated laterally, as it were, from its natural position, being suspended from about the middle of the transverse cord by the heel. The other or left foot had the two large toes wanting, but was less remarkable for its defects of development, than for the abnormal and inverted position of its surfaces and margins, its sole being turned upwards, its dorsum or instep downwards, its external margin inwards, and its internal margin or edge being consequently placed on the outer side. Hence the three last toes which were present, appeared to be situated on the internal instead of the external side of the foot, and their lower surfaces were inverted upwards. At that margin of the foot which was situated internally, but was naturally the external, a slender membrane was perceived prolonged from the heel to the little toe, and continuous above on the back part of the leg, with the transverse cord which passed to the right thigh.*

Case III.—Beclard, in his essay on Acephali, contained in the Bulletins de la Facultè for 1817, (tom. v. p. 513-4, incidentally mentions the case of "a very deformed hydrocephalic fœtus, born in Paris, that had the middle and ring fingers of the right hand mutilated; the middle one, however, still being kept attached to the stump by means of a filament. The legs were covered with reddish phlyctenæ. The left presented at

* This membrane and the different parts of the left hand are very imperfectly shown in the accompanying drawing, but they are equally defectively represented in the original full-length view of the fœtus given by Lagorsky.

its superior part a transverse cut that penetrated down to the bones, and resembled that which would have been produced by a tight ligature. The two opposite sides or surfaces of this indentation were both cicatrized and almost touching one another. It is evident," Beclard adds, " that if this fœtus had remained in utero for some time longer, it would have been born with an amputated and cicatrized leg, the remains of which might have been found in the liquor amnii."

Case IV.—In a dissertation " Ueber Mangelhaste Bildung der Extremitaten," by Albert F. Veiel, printed at Tubingen in 1829, among the list of cases given of defective development of the extremities, reference is casually made to an instance recorded in Froriep's Notizen, Bd. XII. p. 26, of a fœtus " whose left foot was separated during pregnancy from the bone, and the forefoot was born by itself quite healed;" (*der linke Fuss während der Schwangerschaft sich von dem Beine ablöste, und der Vorderfuss für sich, bereits geheilt, geboren wurde.*) I regret that I have not an opportunity of consulting the volume referred to, in order to ascertain the more particular details of this case.

Case V.—In his " Adversaria Medico-Practica," tom. ii. p. 79, Ludwig has described and delineated a child that was born wanting a great part of the left upper extremity, but otherwise well formed. The arm appeared amputated about the middle of the humerus. To the rounded stump two soft papillæ were affixed, consisting of prolongations of the skin only, and a little fat. One of these papillæ is shewn in the drawing given by Ludwig, and is represented as having a slight indentation or small cicatrix on its apex. On dissecting the mutilated arm, its blood-vessels and nerves were found to become contracted as they approached its lower extremity, and at last were changed into a kind of fibrous tissue. The muscles of the arm appeared to become degenerated, like the blood-vessels and nerves, into a kind of fibrous or cellular structure, as they reached towards the surface of the stump. The os humeri was natural at its

upper part, but below it became somewhat broader and flatter than natural, and terminated in a rounded extremity with two slight lateral projections or tubera, which represented imperfectly, and on a very diminished scale, the condyles of this bone.

I have myself had an opportunity of examining, during the present year, three instances of amputation of the forearm in utero, somewhat similar to that of Ludwig, but the mutilated extremity at the point of division presented, in each of them, in a much more marked degree, the exact appearance of an artificial stump, having the usual depressed and puckered cicatrices upon their surface, instead of the prominent and projecting ones met with in his case. In all the three instances to which I refer, no deformity whatever existed in any part of the body except in the mutilated arm; and the subjects of the cases belonged to families, all the other members of which were perfectly formed in every respect. The following are some of the more interesting particulars relative to each of these three cases.

Case VI.—Mary Kerr, at Dauffington near Queensferry, a fat and healthy child, now fourteen months old, was born wanting the right forearm and hand. The forearm is deficient from about half an inch below the elbow, the insertion of the biceps being left, and the elbow joint perfect, as shewn by the free motions of the part. The remaining upper portion of the forearm presents exactly the appearance of a very full and neatly-formed stump, the soft parts affording a large cushion which covers and even overlaps the bones for half an inch. The situation of the two bones, however, is still well marked by two deep, puckered, converging cicatrices, seen on the lower surface of the stump, and leading down at their apices to the divided ends of the ulna and radius. The mother states that the arm presented the same appearance of perfect cicatrization at birth.

Case VII.—Mary Penman, a healthy child of between two and three years of age, living at Kinnaird in the neighbourhood of Falkirk, was born wanting the right forearm from nearly the same point as in Case VI. The stump is less full and fleshy than in Kerr's case, and the divided ends of the bones of the forearm are consequently not so deeply buried in the soft parts. The truncated extremity of the ulna is felt somewhat enlarged, and the enlargement seems more on the anterior than on the posterior surface of the bone, so as to afford an useful fixed point for the child in her attempts to hold or move any object with the mutilated arm, which she is reported to use already with great activity. The portions of the upper parts of the ulna and radius, that are present, seem not to be united together by bony matter, but can be made to move upon one another. On the surface of the stump there is an appearance of puckered cicatrices over the ends of the two bones, one of them being deep and depressed, like the usual sunk cicatrix of the umbilicus, but less in size, and another much shallower, and containing in it a very small flattish mass of the consistence of cartilage. From one of these cicatrices a slight furrowed line runs forward for a short distance, to what almost appears to be third cicatrix on the anterior part of the surface of the stump The mother states that at the time of birth, a part over the ulnar or inner side of the stump was for a small extent raw or uncicatrized, but it skinned over in the course of a few days afterwards.

Case VIII.—Margaret Forrester, Grangemouth, a married woman about forty-two years of age, and the mother of three well formed children, two of whom are at present alive, wants the right forearm and hand, from about an inch and a half or two inches below the elbow, a deformity which has existed since birth. The stump is not very fleshy, but she uses it with extraordinary adroitness, being able to sew, wash, and perform every other kind of ordinary household duty with it. The portions of the upper parts of the radius and ulna which it con-

tains, appear to be firmly united together at their corresponding surfaces by bony matter. The surface of the stump in respect to cicatrices nearly resembles, in almost every particular, that of Mary Penman's arm, only the cicatrices are not so very deep, and have more the appearance of perfectly natural skin, owing probably to the more advanced age of the person. Indeed the cicatrix over the end of the radius is more distinguished by the natural sulci of the skin at that part all radiating from it, as from a central point, than by any perceptible difference there is in the structure of the cutaneous texture itself. In the cicatrix over the site of the ulna, there is a small circumscribed cartilaginous thickening. Mrs. Forrester is not herself informed as to whether any difference of appearance was observed in the cicatrices or stump at the time of birth.

In none of the three cases which I have just described, were any remains of the absent forearm or hand observed, and most probably they were not looked for or ever suspected to exist among the secundines, by the attendants,—a subject which I shall again have occasion to revert to in the course of the subsequent remarks.

Case IX.—I have had repeatedly an opportunity of examining another case of considerable deformity of the hand, which is not improbably referrible to the kind of congenital mutilations now under consideration. Helen Crawford, a native of Bathgate, and now a child of between two and three years of age, was born wanting entirely the index and middle fingers of the left hand. The skin at that part of the hand from which these fingers should have projected is smooth, and the extremities of their two metacarpal bones are felt lying beneath it, the second being shorter than the first. The ring and little fingers of this hand are very intimately coherent with one another; the thumb is small, and has, opposite apparently to the situation of the joint between its first and second phalanges, a remarkable circular depression or indentation in it, of about one-third of the transverse diameter of the thumb in depth. The parts are

alleged to have presented exactly the same appearance in all respects at birth ; and no remains of the absent fingers were observed. The left arm has not kept pace with the right in growth, and is now nearly an inch shorter than it. The different parts of all the other extremities are naturally formed.

Case X.—In the Acta Academiæ Cæsareæ, tom. v. p. 181, an example of malformation of the hands is recorded by Aculoth, which is interesting from the similarity that it bears to the preceding case. There is no particular description of the parts given by the reporter ; but from the drawing appended to the brief notice of the case, it appears that the thumb, index, and little fingers of the left hand, and the thumb and little finger of the right, were normally and perfectly formed. The middle fingers of both hands are altogether wanting. The left ring finger seems as if deprived of its last phalanx and nail, and about the situation of the joint, between its first and second phalanges, it is almost completely divided by a very deep circular depression ; so that the body of the second phalanx, which is of the natural size and thickness, appears to be fixed to the extremity of the first by a slender neck or pedicle of skin only. The ring finger of the right hand presents at the same point an equally deep circular furrow ; and, in addition, has what seems to be a diminutive third phalanx, unprovided with a nail, attached to the extremity of the second by a similar slender pedicle. In the situation of the right index finger there is seen only a small roundish body, about the size of this third phalanx of the ring finger, and connected to the metacarpus by a similar narrow neck or isthmus of integument.

In judging in any doubtful case of deficient or deformed extremity, whether the defect which we observe be the result of a want of the original development of the absent parts, or the effect of a separation or amputation in utero of these parts after they had been once actually formed, there are some circumstances which will in most instances, I believe, be found a suf-

ficient guide to enable us to come to a satisfactory decision of the question. When the malformation of the limb is the result of an arrest or deficiency of its original development, it would seem to be very generally observed, that one or more parts of the pertaining hand or foot, forearm or leg,—sometimes, it is true, a very small part only, as portions of a single finger or toe, or some bones of the carpus or tarsus,—are affixed to the extremity of the defective limbs, these more peripheral parts appearing very early in the natural process of the development of the extremities, and not unfrequently being seen to exist alone in those cases of their arrested or deficient development, in which no other part whatever of the limb is present, in consonance with the law of the development of organs from their circumference to their centre, pointed out at such length by M. Serres, but probably not by any means so universally applicable as he himself seems to believe it. In the cases, again, in which the deformity of the extremity has been produced by the process of spontaneous amputation, the end of the mutilated limb will be found to present the appearance of a common artificial stump, in so far that the soft parts and bone are perfect up to the point of separation, and have no parts of the limb, that are naturally situated anterior or distal to this point, attached to them. It is evident that when the deformity is confined to the fingers or toes alone, (unless it happens that the third phalanx, as characterized by its nail, happen to form the part of the finger or toe that is present, and thus authorize us to refer it to an arrest of development,) the above criteria will not be sufficient to enable us to distinguish between malformations originating in that cause, and mutilations produced by spontaneous amputation. In most such cases we must, I conceive, be chiefly guided by the appearances of the surface of the stump at the time of birth, and the fact of the adjoining fingers or toes presenting one or more deep circular depressions, as indicative of the process of separation going on in them. It is principally, at least, from this latter circumstance that I have ventured to include the

ninth and tenth cases among those of spontaneous amputation; at the same time it must be allowed, that the absolute diminutive bulk of the whole left arm, and, I may add, of the left side of the chest also, in the girl Crawford, cannot but be considered as a strong argument in favour of the deformities of her hand depending in some degree on a defective development.

In the three first cases which I have quoted, (and the same remark partly applies also to the ninth and tenth,) the division of the limbs and fingers at the strictured parts, was not so complete as to have produced their entire separation. The same was the fact in regard to both the instances seen by Dr. Montgomery. In Mr. Watkinson's case, in one of Chaussier's, and also, I presume, in the instance in Froriep's Notizen, referred to by Veiel, the division was complete at the time of birth, and the amputated portions of limb were discharged along with the fœtus from the uterus. In other examples, as in that given by Ludwig, and in the cases that I have brought forward of Kerr, Penman, and Forrester, the evidence of the amputation has been so far incomplete, that the separated portions of limb were not observed by the attendants;—a point in the history of all the cases of mutilated fœtal extremities that Haller could find on record at the time at which he wrote, and which he has collected in his Elementa Physiologiæ, (tom. viii. p. 139,) that led him entirely to reject the idea of such deformities ever depending on spontaneous amputation in utero. "No author," he observes, (p. 140,) "has cited an instance in which the amputated hand, or other limb, was found in the membranes, separated from the body of the fœtus; and yet in the more advanced terms of pregnancy the fœtus is of no small size, and no powers of nature could annihilate the absent or separated portion of limb." With reference to this remark, the two first cases that I have brought forward appear to be very important, because the circumstance of the separated part of the limb being sometimes stopped in its development at a comparatively early stage of intra-uterine life, as seen in the left leg and foot in Schaeffer's,

and in the right in Lagorsky's case, may serve to explain how an oversight of it might very easily be made. For if the cord or membrane uniting this undeveloped part of the limb to its corresponding extremity, should happen to become completely divided, either during the course of pregnancy—in the act of parturition—or immediately after birth, it is evident that on account of the arrested formation and minute size of the part itself, it may be difficult to detect it among the secundines and discharges, even though it were carefully looked for.

The cases described by Schaeffer and Lagorsky appear to support strongly the idea entertained by Dr. Montgomery, that the amputation of the limbs of the fœtus is, in some, if not in all instances, the result of the constriction of a cord or ligature thrown round the limb at the point of disjunction; and the language which Beclard employs in the description of his case (see Case III.) would almost seem to show, that he held a somewhat similar opinion with regard to the agency employed in the process of separation. And here, it may not be unworthy of remark, how similar in some respects the ligatures or cords are in the two cases referred to; for if we suppose, what was very probably the case, that in Schaeffer's child, the portion of cord attached to the second toe of the right foot (a, Fig. 3) was originally adhering by its loose extremity to the small eminence, (d, Fig. 4,) seen on the superior part of the left and undeveloped foot; this left foot would, before the attachment was lacerated, exhibit an appearance of connexion and suspension, analogous to that presented by the right foot in the monstrous fœtus described by Lagorsky.

As to the nature of the bands or cords which constitute the constricting ligatures, there seems no reason to doubt but that they are, as Dr. Montgomery has suggested, formed of organized lymph, and that this lymph has been effused by inflammatory action is, I am inclined to think, in the highest degree probable. No one would now attribute such morbid bands or cords, if they were found in other parts of the body, sa in the cavities

of the chest or abdomen, to any other morbid action than that of inflammation; and that the cutaneous texture of the fœtus in utero is liable to inflammation attended with the effusion of organizable lymph is known from the circumstance of different parts of the surface of the fœtus being occasionally found adhering, either directly or by the medium of false membranes, to the amnion covering the placenta, as seen in a number of cases which I have elsewhere collected, (Essay on Diseases of the Placenta, Edinburgh Med. and Surg. Journal, vol. xlv. p. 307,) to some part of the umbilical cord, as described by Pole, Meckel, Velpeau, and others, or to other parts of its own body, as observed in the case mentioned by Morlanne, (Gardien Traité des Accouchm. tom. ii. p. 173.) Further, that organizable lymph, when effused by inflammatory action between two contiguous parts of the surface of the fœtus, is capable of being changed into pseudo-membranous bands or cords, similar to those observed in Dr. Montgomery's first case, and in the cases described by Schaeffer and Lagorsky, appears from the circumstance of lymph occasionally assuming this form, when effused between the contiguous points of such surfaces as have a free motion on one another, as is sometimes seen in the bands of false membrane found stretching from one point to another of the cavities of the pleura, pericardium, and peritoneum. Besides it has been particularly noticed, that in some of those instances in which one or more points of the surface of the fœtus has been morbidly united to the membranes or umbilical cord, that the effused organizable lymph forming the band of connexion between the adherent parts has, probably from the free motions of these parts upon one another, and the consequent stretching to which the pseudo-membrane was constantly subjected, taken more or less completely that form which the cords in the above cases presented. Thus Meckel, (Handbuch der Pathologischen Anatomie, Bd. II., s. 138,) mentions a preparation in his possession, shewing, in a fœtus of the sixth month, the dura mater protruded through an enlarged occipital foramen,

and morbidly united to the umbilical cord through the medium of a strong thread (*starker faden*) of membrane, of six inches in length. Gurlt (Lehrbuch der Pathol. Anat. der Haus-Säugethiere, Bd. II., s. 133, and Tab. VII. Fig. 1) has described and represented a variously malformed fœtus of the goat, which had been adherent at different points of the surface of the amnios. One of the bands of adhesion, of about an inch and a half in length, is represented as arising from the upper lip, and has exactly the appearance of a cord or thread in its middle portion, and a more dilated and membranous form at either extremity.

In the present state of our knowledge with regard to the diseased conditions to which the fœtus is subject, it would be difficult, or indeed altogether impossible, to determine what may constitute the active or exciting cause of the apparently local inflammation which gives rise to the exudation of that organizable lymph that constitutes the constricting pseudo-membranous cords, in such cases as those under consideration; but there are some facts connected with the *seat* of the adhesions, or origin of the cords, that may ultimately lead to some knowledge on this point, and which seem, at least, in the mean time, not unworthy of being particularized. In the upper extremities, the root of the fingers or middle of the hand appears to form a very frequent locality for the seat and effects, if not for the origin, of the inflammatory action which gives rise to the constricting bands of false membrane. Thus in Dr. Montgomery's first case, the threads of lymph were placed as a complete ligature round the middle of each hand, causing a distinct depression in the constricted part; in Lagorsky's case, all the fingers of the left hand were amputated at their roots, and the inside of the thumb of the right hand was partially adhering to the side of the palm; in Schaeffer's child, the thumb of the left hand was united in the same manner to the metacarpal part of the index finger, and the two last phalanges of the index and middle fingers were wanting, while the middle, ring, and little

fingers of the right hand were held together by a common membrane; in the girl, Crawford, (Case IX.) the ring and little fingers of the left hand are coherent, while the index and middle fingers are separated from their roots, and the thumb at its second joint has a circular indentation in it, which existed at birth, and was not improbably formed by the constriction of the ligature that had separated the two last mentioned fingers. In Beclard's hydrocephalic fœtus, the right ring finger was wanting, and the middle one nearly separated by a deep circular depression; and lastly, in Aculoth's case, the ring fingers of both hands were very deeply divided with similar circular depressions or furrows, the two middle fingers were entirely wanting, and the right index finger nearly so.

The upper third of the forearm has now formed the seat of the amputation in five cases, viz., in two of Chaussier's, and in three of the instances (Cases VI. VII. and VIII.) which I have met with. While the lower third of the leg seems to have been the most frequent point for its occurrence in the lower extremities, it having been observed now in this situation in Dr. Montgomery's two cases, in Mr. Watkinson's, in Schaeffer's, and in the left leg of the monster described by Lagorsky. In the instance mentioned by Beclard, the seat of stricture was at the upper part of the leg; in the right lower extremity in Lagorsky's case it was placed above the knee; and in Ludwig's and one of Chaussier's cases, the amputation had occurred in the course of the humerus.

Allowing, what there seems every reason to believe, that the pseudo-membranous threads or bands found at the seat of separation or amputation of the fœtal limbs, form the active physical agents employed by nature in the production of this effect; it may still be allowed difficult to conceive how they should produce such a strong constriction, and consequent atrophy, of the member at the point of ligature, as leads not only to the partial but even to the entire disjunction of the limb at that point. With reference to this subject, it is necessary to recol-

lect, how readily the atrophy or interstitial absorption of any living texture is produced by the application to it of a continuous and strong pressure. Besides, in the earlier months, in particular, of intra-uterine life, the limbs of the fœtus are so slender, and their component tissues so soft, as to afford great facilities for the disjunctive action of the constricting pseudo-membranes. We must further remember, that supposing the organizable lymph, forming one of these constricting false membranes or bands, to be once effused, so as to have its two extremities attached to two parts of the body, more or less distant from one another, as to the parts of two limbs, it is evident that the texture of the pseudo-membrane must itself soon become stretched, and at the same time compress more or less forcibly those parts or surfaces of the fœtus, over or around which it may happen to pass, in proportion as the two points of the body forming its origin and insertion become gradually more and more separated from one another, in the regular progress of development. The sudden movements also of the fœtus, in the latter months of pregnancy, may contribute in some degree to the same effect.* This stretching of the cord of pseudo-membrane is capable of producing a constriction ; but also

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* It is curious, and, at the same time, as far as regards the mechanism of the process, not unimportant to connect our knowledge of the method employed by nature for the separation of the limbs of the fœtus, with the mode of amputation without bloodshed, practised in the fourteenth century by Gui de Chauliac, by applying to the joint that was situated more immediately above the seat of disease, a ligature so tight as ultimately to cause all the portion of limb below the point of constriction to drop off. The same method of amputation was again brought under the notice of German surgeons, towards the end of the last century, by Wrabetz and Ploucquet, and recommended by the latter as more particularly adapted to the case of emaciated and timid subjects.—(See Sprengel's Histoire de la Medicine, tom. vii. pp. 313 and 333 ; Cooper's Dictionary of Surgery. During last year, in the experimental rooms of M. Amussat at Paris, I had an opportunity of seeing M. Mayor, the distinguished surgeon of Lusanne, attempt to cut off the limbs of dogs, by forcibly tightening around them a circular ligature of strong wire.

in some circumstances an elongation of the limb; as seen in Schaeffer's case, (Fig. 4. C.) Upon the right foot of the fœtus described by Lagorsky, an interesting effect has been produced apparently by this same cause. The pseudo-membrane attached to it seems to have been originally affixed along the inner side of the heel, and is stated to have stretched from thence as far forward as the little toe. In its course to the opposite extremity it had passed round the outer and across the forepart of the leg; and when stretched, had, in consequence of its connexions and course, produced the effect of turning the included portion of leg and foot half round upon its axis, so as to bring the inner ankle to the situation of the outer, and to invert the surfaces of the foot in the manner before mentioned. (See Case II. and Fig. 6.) Both in the child described by Mr. Watkinson, and in Dr. Montgomery's second case, the foot which was not the seat of the amputating process was turned partially inwards.

The sketch given in the plate of the left leg in Lagorsky's case, may serve also to illustrate a remark made by Dr. Montgomery, as to how it happens that in cases of amputation of the fœtal limbs, the surface of the stump, and even of the amputated portion of the extremity, seem nearly or entirely cicatrized; for, it is evident, that by the constricting ligature gradually carrying in before it, at the point at which it encircles or compresses the limb, a duplicature or process of the cutaneous texture, and from this texture constituting (as it certainly appears to do) one of the last that is removed by the disjunctive atrophy; both sides of the deep indentation or furrow, formed at the seat of constriction, must be found regularly covered over by the elongated and depressed integuments as far almost, in most cases, as the bone itself; and even the small portion of the stump which is left raw by the division of the last parts, may be observed in some instances invested with a true cicatrix at the time of birth; provided the separation of the limb has taken place at such a period before the expulsion

of the child from the uterus, as to allow of this cicatrization being completed.

The view that has been taken of the disjunctive action which we have attributed in the preceding remarks to the forcible and gradually increasing constriction of the bands of pseudo-membrane upon the fœtal limbs, seems confirmed, in no inconsiderable degree, by what is occasionally observed to occur when the same kind of constriction is exercised upon any part of the fœtus by the umbilical cord. From the facility with which the smooth surface of the cord moves over that of the fœtus, it is of extremely rare occurrence that the convolutions of the former around the neck, body, and extremities of the latter, ever come to act as a constricting or compressing agent upon the parts over which they pass, although such an effect has been occasionally observed, while the surface of the cord still remained healthy, when its coils have accidentally become so very firmly entangled and knotted with one another, or with the fœtal limbs, as to prevent its own motions, as seen in a remarkable case represented and described by Van de Laar, in his Observationes Obstetrico-Medicæ, (p. 41, and Tab. II.) in which the arms of an aborted fœtus were curiously displaced, and distorted on account of their being thrown transversely across the back, and twisted in that position, with some duplicatures of the navel string. When the surface of the cord, however, becomes, as occasionally occurs, morbidly adherent at some point to the surface of the fœtus, the state of matters is necessarily much changed; the portion of cord between the umbilicus and point of adhesion is, as it were, fixed, and the parts which it happens to be stretched over or to encircle in its course, are liable to become compressed and constricted by it, during the progress of the natural enlargement of the body, in the same manner as we have already seen them to be by the ligatures or bands of pseudo-membrane. Thus Meckel (Pathologischen Anatomie, Bd. II. s. 137) mentions the case of an aborted fœtus of the third month, whose umbilical cord, after being indented to the

depth of half an inch into the parts of the right scapular region, stretched from thence downwards to the right thigh and leg, to which it was intimately united by morbid adhesions. Wrisberg (Sandifort's Thesaurus, tom. iii. p. 235, Tab. II. Fig. 5) has delineated and very minutely described a case which is still more in point, in reference to the present subject. In a deformed fœtus of the fourth month, the umbilical cord, on leaving the abdomen, ran first over the left shoulder, and around the back of the neck, and then came to encircle completely the right upper extremity immediately below the shoulder. To all these different parts it was morbidly adherent, and at the point at which it encircled the right arm, it indented itself deeply into the subjacent soft parts, (*carnes subjacentes valde stringit.*) In its subjacent course the cord, after running again over the left shoulder, returned a second time to the right arm, and crossed over it above the elbow, impressing another furrow or indentation upon it at the point of contact. In this case we have an example of the process which produces the spontaneous amputation of the limbs of the fœtus, going on at two different points in the same arm, and it differs from the instances previously detailed in this respect only, that the constricting and dividing agent was not, as in them, a band of false membrane, but a portion of the umbilical cord.

At one time I was inclined to believe that spontaneous amputation of the limbs of the fœtus might be occasionally produced in another mode from that which we have above considered, and this opinion was suggested by the following case. In 1834, a boy, at that time of seven years of age, was exhibited in Edinburgh, wanting both arms and hands. From the right shoulder one finger containing four phalanges, and from the left two other shorter fingers protruded. The two fingers attached to the left shoulder were situated vertically over one another, and the inferior presented at one point, in the course of its first or proximal phalanx, a great and sudden narrowing or contraction. On making some further inquiries, it appeared that this

last finger had been fractured some years before by a fall, and had gradually from that time become more and more atrophied at the fractured part, so much so that if the same process continued much longer, it seemed not improbable that the pedicle or neck of integuments attaching the finger might ultimately come to be completely divided, and consequently all the portion of the member anterior to it allowed to drop off. The circumstances of this case readily suggested the idea that the similar constricted appearance, and occasional complete amputation of the limbs of the fœtus, might possibly, in some instances, originate in the same cause. We know from various accurately recorded instances, that fractures do occur in the bones of the fœtus in utero, as an effect of physical injury, and also spontaneously, (if we may trust to the cases observed by Chaussier and Baudelocque,) and it seems possible, at least, that under some circumstances this fracture may be attended with such lesions of the surrounding soft tissues, as may give rise to a kind of disjunctive atrophy or absorption, similar in its nature and effects to that which had occurred in the finger of the boy, and perhaps in the textures of the fœtal limb, still more active and speedy in its operation. At the same time it must be admitted, that in none of the instances hitherto recorded of fractures of the bones of the fœtal limbs, does such an atrophy of the surrounding soft parts appear to have been observed, if we may not be allowed to except the case given by Amand, who found in a fœtus of the fourth or fifth month the bones of the forearm, thigh, and leg separated and mobile, as if they had been fractured by design, and the parts seemingly held attached by the medium of the integuments only. (Nouvelles Observations sur la Pratique des Accouchmens, p. 92, obs. viii.)

P. S.—In his first communication, (Dublin Journ. of Med. Science, vol. i. p. 143,) Dr. Montgomery has referred to a passage in Haller's Elementa Physiologiæ, (tom. viii. p. 135,) in which that author casually alludes in such a way to some expressions of Roederer, as led Dr. Montgomery to suppose, that

this last writer might have described the compression of the limbs of the fœtus with cords of organized lymph. Having access to the work of Roederer referred to by Haller, I beg to subjoin the passage alluded to, only remarking, that the language of the author is not so definite as to leave the matter entirely without doubt. It is contained in an essay, "*De vi Imaginationis in œtum Negata*," in his Opuscula Medica, p. 120. After speaking of the supposed resemblance of certain deformed human fœtuses to apes, he observes, that the comparison is not so far fetched, for "*femora abdomini applicantur, reflectuntur crura, ad faciem eriguntur manus, retractis cubitis. Inquirens in structuram corpusculi ligamenta strictiora notavi, artusque compressos.*"

ART. XIV.—*Notice of a newly discovered Peculiarity in the Structure of the Uterine Decidua, or Decidua Vera.* By W. F. MONTGOMERY, M. D., Professor of Midwifery to the King's and Queen's College of Physicians in Ireland.

[Read before the Medical Section of the British Association, at Bristol, August 25, 1836.]

IT would be an unprofitable occupation of the time of the Association to enter into any thing of a general or detailed account of the structure and relations of the decidua, which are so familiar to the profession. I shall, therefore, take for granted that my hearers are in full possession of the different points already well known on this subject, and confine myself exclusively to a brief notice of a peculiarity in the structure of this product, which, as far as I am aware, has never been described, although perhaps one of its most important and interesting features. About four years ago, while preparing the component parts of a human ovum, in the third month, for lecture, I observed that when the decidua vera was immersed in water, with its uterine surface uppermost, there appeared amongst the floating and

shred-like processes which covered it, certain small circular openings, which at first I took to be merely foramina in the membrane; but on attempting to pass the point of a fine glass rod through one of them, I found it to be a cul-de-sac: being thus incited to ascertain how the matter really was, and examining carefully then, and having repeated the examination frequently since, I have fully satisfied myself and others who have examined the part with me, or to whom I have exhibited it in my lectures on embryology, both of the existence and peculiar characters of the structure about to be described. There are on the external or uterine surface of the decidua vera a great number of small cup-like elevations, which project from it like little bags, the bottoms of which are attached to, or embedded in the substance of, the decidua; they then expand or belly out a little, and again grow smaller towards their outer or uterine end, which is, in by far the greater number of them, an open mouth, when separated from the uterus: how it may be while they are adherent, I cannot say. Their form is circular, or very nearly so, they vary in diameter from $\frac{1}{12}$ to $\frac{1}{6}$ of an inch, and are elevated to about $\frac{1}{12}$ of an inch above the surface to which they adhere;* in the way of comparison, I would say that they were miniature representations of the suckers of the cuttle fish. They are not confined to any one part of the decidua, but are usually most numerous and most distinct in those parts of it which are not connected with the capillary rudiments of the placenta, and at the period of gestation which precedes the formation of the latter (the placenta) as a distinct organ; hence the best time for examining them is up to the third month. In the advanced periods of gestation they are not to be found; at least, I have not seen them then. I am ready to confess at once, that I am not prepared to offer any decided opinion as to the precise nature or use of these decidual cotyledons, for to that name, their form, as well as

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* An accurate engraving of these decidual cotyledons will appear in a work on Pregnancy by Dr. Montgomery, now on the eve of publication.

their situation, appear strictly to entitle them; but from having on more than one occasion observed within their cavity a milky or chylous fluid, I am disposed to consider them reservoirs for nutrient fluids, separated from the maternal blood, to be thence absorbed for the support and development of the ovum. This view appears strengthened when we consider, that at the early periods of gestation the ovum derives all its support by imbibition, through the connexion existing between the decidua and the villous processes covering the surface of the chorion.

ART. XV.—*Case of Fracture of the Neck of the Femur.* By W. H. PORTER, one of the Surgeons to the Meath Hospital, and Lecturer on Anatomy and Surgery in the Medico-Chirurgical School, Park-street.

It is familiarly known that a difference of opinion exists amongst some of the highest authorities, on matters connected with surgical science, as to the symptoms that characterize different forms of fracture of the neck of the thigh bone,—as to the possibility of osseous union between the fragments, and the circumstances that conduce to protract or prevent so desirable a result. It is also known, and must be regretted, that so few dissections have been made after the very recent occurrence of the accident, obviously because, however it may shorten the patient's existence ultimately, it does not immediately or even speedily destroy life: and it is quite evident that an examination, after an interval of six or seven months, will not be satisfactory in explaining the derangements that the fracture occasioned at the time, or the condition of the bone that preceded, and it may be, predisposed to it. Under these circumstances, no apology is necessary for recording the details of a single case; for, by omitting to notice an infrequent occurrence, or by waiting until the result of our observation should be sanctioned by

an extended experience, many valuable contributions to pathology have been lost, and many facts passed over, which, being collected and compared by some future observer, might lead to results unattainable in the life of any single practitioner.

Mary Kane, æt. 69, admitted into the Meath Hospital, on Thursday, 18th February, 1836.

On the previous day, whilst walking across the road opposite her dwelling, she made a false step, and fell with her hip against a stone; she became unable to rise, and lay for some time exposed to the inclemency of the weather, until found by some passengers, by whom she was carried into the house, whence, on the following day, she was conveyed to hospital on a cart. On being examined by the resident pupil, it was found that she *could* bend the knee, and draw up the foot by the action of the muscles of the limb alone, and without the assistance of her hands.* Any attempts to rotate the thigh inwards, or to perform abduction, occasioned great pain, and the suffering was extreme when the foot was pressed upwards.

When I saw her, the limb was lying perfectly powerless: she could not draw it up, or indeed perform with it any motion whatever. It was not shorter than the other; neither was the toe in the smallest degree everted. She experienced great pain

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* I mention this on the authority of the resident pupil, it being entirely opposed to my own experience. Doubtless, persons have been able to walk with the neck of the thigh bone fractured: "Des sujets ont peu se marcher pendant quelques jours avec le secours d'un baton, avant que le deplacement ne se fut effectuè."—(*Boyer.*) This may be explained by the fragments being engrained in each other, and by the orbicular ligament which covers the neck, remaining unbroken. I may remark here, that even this symptom must be rare, for I have never seen it but in one instance, a young man who had been subjected to immense violence; and, moreover, that although able to support himself when up, he was unable to raise himself or even bend up the limb. I do not entertain an opinion, that the aged female can stand for a moment, after having sustained this kind of injury.

on the joint being examined, or pressure made in its neighbourhood, so that I did not press the inquiry farther, as she was in a very debilitated condition; and I knew that if the neck of the bone was broken, a very short time would be sufficient to render the nature of the accident evident.

On the evening of the following day she died, apparently from the effects of the shock on the entire system.

Sectio cadaveris.—On viewing the body as it lay, the injured limb seemed to be from a quarter to half an inch shorter than the other; certainly it was not shortened to a greater extent, and the foot was in a state of semi-eversion. It still rested on the heel, and not on its external side, as it usually does in cases of fracture of the neck of the thigh.

The entire articulation was removed with the greatest care, and with so little violence, that it was not known with certainty until afterwards, (when the capsule was opened,) that the bone had been really broken. The capsule of the joint was found perfect and entire. On dividing it posteriorly to some extent, it was discovered that a fracture of the neckof the femur had taken place in a more immediately transverse direction than I had ever before observed: the situation of the fracture being close to the head of the bone, and of course entirely within the capsule. However, the fragments were not greatly displaced, being held together, not so much by the capsular ligament, which was quite relaxed, as by the synovial membrane and periosteum. These latter were partially detached, in such wise that a portion of the inferior surface of the neck of the bone was completely denuded: but the fibres removed from the bone were gathered together into a bundle forming a kind of ligament of a triangular shape, the base being at the edge of the head, the apex at the point of reflexion of the synovial membrane on the capsule. This appeared to be very tense and strong, and offered considerable resistance to a separation of the faces of the fragments one from the other.

There was not a drop of blood within the capsule, nor effusion of any description of fluid whatever.

The synovial membrane was generally red and vascular; much more than is ever observed in a healthy or uninjured joint: but this increase of vascularity was more apparent in some parts of the membrane than in others; that covering the ligamentum teres was very red, but the intensity of colour was most remarkable in the triangular adventitious ligament already mentioned.

The cartilage of incrustation was also changed in colour. It exhibited small patches of a very pale pink tint, evidently deeply seated in the structure of the part. Perhaps the best mode of endeavouring to describe this appearance is by *supposing* the bone underneath to be greatly increased in vascularity, and the red colour to be seen through the cartilage. I had no means of ascertaining whether it was situated *within* the cartilage or not, as I wished to have a drawing taken of the appearances, an object which a section of the bone or other minute examination would have interfered with. I notice this appearance particularly, because I think it bears in some respects on the supposition, that in this kind of fracture the superior fragment is deficient in vascularity; a supposition that has been adduced by very high authority as explanatory of the cause why osseous union does not take place in this kind of fracture.

The thigh bone of the opposite side was then examined with a view of ascertaining the nature of the changes of shape and structure that take place in advanced life, and predispose to or facilitate the occurrence of the accident. A longitudinal section was made of it and compared with a similar section of the bone of a very aged man,* and of one taken from a young female apparently of the age of twenty six or twenty seven years.

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* The age of this subject was not ascertained, but it was probably very advanced, for he exhibited specimens of the senile disease of the joints, which I have never seen under the age of 60, generally far beyond it.

The necks of the bones in the aged subjects were thicker and more clumsy in appearance: their direction with respect to the shafts was more at a right angle; I could not ascertain that they were increased in length in the smallest degree.

The internal structure of the different bones was then examined; that of the aged female first. The cells of the cancellated tissue, generally, throughout the head, neck, and superior extremity of the femur were enlarged: in some places portions of this tissue seemed detached from the rest: and immediately at the neck, for more than half its thickness, the earthy material was entirely removed, and its place occupied by a deposition of soft fat or marrow of a yellowish grey colour. All the marrow was of this colour, and apparently deficient in vascularity. The cancelli, even where they appeared most perfect, readily broke down under the gentlest pressure of the finger. As far as I could observe, there was no alteration in size or thickness of its external walls.

It may be interesting to mention that the entire osseous system of this female was weak and fragile: the fibula gave way under the pressure of a finger and thumb, and was crushed as easily as an eggshell.

The bone of the old man exhibited a nearly similar change of colour in the marrow: its cancelli were also somewhat enlarged, but not at all to the extent observed in the previous case, and they were firmer and more resisting, requiring considerable pressure to break them down.

The bone of the young female exhibited a dense and close cancellated tissue, the cells very small, and filled with marrow of a red colour, and very vascular, which contrasted remarkably with the pale yellow tint observed in the other bones. Its cancelli were very strong, and resisted the utmost pressure made by the thumb.

I have not seen in any author the dissection of a case so very recently after the infliction of the injury; and I attach the more value to it, because it brings us acquainted with the con-

dition of the osseous system almost at the moment of the accident, and before any lapse of time, confinement to bed, or change of diet or of habits could operate the smallest change. It certainly adds another link to the chain of evidence which goes to prove, that in fractures completely within the capsule the limb is not at all or but slightly shortened, and it shews that the chief obstacle to such shortening is the periosteum of the neck and the reflected synovial membrane. These facts have, however, been already so amply dwelt upon by others that it would be useless to enlarge on them farther.

It must be observed, that a vast difference obtains in the opinions of practical men as to the probable results of fractures of the neck of the thigh, and the possibility of osseous union being accomplished. Some, amongst whom are Boyer and Earle, suppose that if this fracture was placed and maintained under proper restrictions, in fact under circumstances to which other fractures are subjected, a perfect bony union might and would be accomplished. Others, again, deny the possibility of such an event; and Sir A. Cooper (I believe, for I quote entirely from memory) states, that the fracture external to the capsule is susceptible of perfect union, whilst that within it is not. The actual discrepancy that exists in the termination of cases of this accident, is every day made obvious to hospital surgeons; and I find that the unexpected success which Professor Colles had met with in some unpromising cases of fracture of the neck of the femur, and the equally unexpected disappointments which he encountered in other apparently favourable cases, induced him to turn his attention to the consideration of this injury, and led to the publication of one of the most valuable papers that exists upon this subject. A desire to assist in elucidating so difficult a point led me to examine the condition of the unbroken bone in my subject; and a suggestion of my friend, Mr. Adams, as quoted in Dr. Smith's excellent essay, induced me to look particularly to its internal structure. Mr. Adams seems to think, that the chief strength of the bone

resides in an arch of compact tissue, which begins small where the globular head joins the under part of the neck, but which gradually enlarges downwards towards the lesser trochanter, and even so low as the middle of the femur. Now, in my preparation of the very aged bone, this arch is as strong as ever: indeed, if there is any difference, it seems to be closer and more compact, than in the bone of the young subject. But the change that has taken place in the internal structure is truly remarkable. Instead of the firm and strong and resisting cancelli, there is only a piece of unresisting fat or marrow; instead of this tissue being red and vascular and highly organized, it is pale and bloodless. It it true, no induction can be drawn from one case, but should similar appearances be hereafter observed, we may be able to account for the frequency of the accident in old persons, and for some of the varieties in its termination. I know one patient who suffered the injury so long back as January, 1814: she recovered with a useful but deformed limb, and still lives. Another, who broke the bone in 1822, stands and walks as strongly as a person can who has one limb some three inches shorter than the other. Boyer mentions some cases "perfectly cured, and leaving the hospital without the slightest shortening of the limb;" the patients were all under the age of fifty years. Such circumstances may have easily given rise to the idea that in the bone of the young and healthy subject union is possible; whilst in the old, where the constitution of the structure is changed,—where it has become deficient in vascularity and poorly organized, the accomplishment of such a process is not to be looked for. I believe, although the latter part of this proposition may be proved by the dissection of numerous old subjects: there is no *evidence* as yet to demonstrate by dissection a perfect bony union in a subject of any age, or where the fracture has occupied any situation either within or without the capsule.

ART. XVI.—*Notice of the Tincture of Acetate of Iron.* By Dr. ALDRIDGE, Lecturer on Natural History to the Dublin School of Anatomy, Surgery, and Medicine.

THE history of this preparation has hitherto been enveloped in very great obscurity. Every chemist had a different method for its preparation; and very opposite opinions have been entertained with respect to its composition. Dr. Barker states in his observations on the Dublin Pharmacopœia, that it is the solution of a double salt, an acetate of the protoxide of iron and potash, but without advancing any arguments in favour of this view, while others have as decidedly asserted, that it is a simple solution of peracetate of iron in alcohol, in the great systematic works on chemistry we have no mention of it. Berzelius* describes a proto-acetate, prepared by dissolving proto-sulphuret of iron in acetic acid, consisting of small green crystals, which decompose readily in the air; and a peracetate obtained by dissolving hydrated peroxide in vinegar, forming a red solution, convertible by evaporation into a brownish deliquescent jelly. He asserts that this salt, dissolved in acetic æther, and mixed with alcohol, is used in medicine. We shall see by and by that none of these descriptions correspond with our tincture.

Every person engaged in the practical details of pharmacy is aware of the extreme uncertainty of this preparation, made according to the pharmacopœial directions. I have myself prepared it apparently in an exactly similar manner at different periods; and at one time succeeded in obtaining a very deep coloured tincture: at another completely failed. We find in every laboratory some particular formula, which is considered infallible by its inventor. These directions vary exceedingly; and it might naturally suggest itself that the products should be very different. According to some, you will never fail if yo

* Traite de Chimie traduit par M. Esslinger, tome iv. pp. 220 et 230.

dry the sulphate of iron: others say, you must add an excess of acetic acid; while in the opinion of others, all that is wanting is the employment of sufficiently strong alcohol. I remember that during my apprenticeship, I was in the habit of preparing a very dark and permanent tincture, by the following process: mixing one part of proto-sulphate of iron, and two of acetate of potash, with some distilled vinegar, and exposing to the air until it acquired a deep brown colour; then drying and digesting in rectified spirit. The theory upon which I founded this process appeared very plausible: I was aware that the proto-salts generally differed from the persalts of iron, by their greenish colour, insolubility in alcohol, and liability to speedy alteration in the air. As the present substance presented a complete contrast to all these characters, I was led to conclude, that it was a solution of peracetate of iron; an exposure to air necessary for peroxidation, consequently appeared requisite; and inasmuch as a neutral acetate should have the oxygen of the acid, bearing to that of the base the proportion of three to one, the addition, under such circumstances, of a certain quantity of acid would be imperative. My faith in the correctness of this view was, however, very much shaken, by an experiment shown to me some time since by my late master and esteemed friend, Dr. Leet, of this city. He introduced sulphate of iron and acetate of potash powdered separately into a bottle; and then pouring on strong alcohol,—immediately corked. The mixture very well agitated was allowed to subside, when a deep Port wine-coloured solution was obtained by decantation. This preparation puzzled me not a little. It was most probably a proto-salt, yet of a red colour,—soluble in alcohol, and permanent in the air. All these circumstances tend to throw a very considerable interest about the history of these compounds. My curiosity was excited to penetrate the mystery which enveloped both the preparation and composition of these tinctures. I commenced a series of experiments, to elucidate the causes which produced this uncertainty, and I found that every step in the

pursuit of these investigations opened unexpected and important views, abounding with novelty and interest.

My first object in following these researches was, to determine whether the tinctures, prepared according to different methods, were identical in properties and composition. For this purpose I prepared several solutions. The two most remarkable were prepared according to the following formulæ:—

No. 1.—An ounce of proto-sulphate of iron; an ounce of acetate of potash; powder separately, and digest in ten ounces of rectified spirit, (alcohol of the shops;) having first well agitated together, allow the sediment to subside, and decant.

The solution thus prepared was of a deep Port wine colour; when diluted with water it remained transparent; was unchanged by exposure to air; potash and ammonia produced a reddish-brown precipitate; and tincture of galls slightly deepened the colour, but produced no black precipitate.

No. 2.—Half an ounce of dried proto-sulphate of iron; an ounce of acetate of potash, rubbed well together, and dried in a wedgewood capsule; then agitated, and digested with ten ounces of rectified spirit; and as soon as the sediment had subsided poured off the tincture.

A very deep and nearly black tincture, which never completely cleared. It threw down a copious brown precipitate when diluted with water; precipitated reddish-brown with potash and ammonia, and turned black with tincture of galls. It was *also permanent in the air.*

By these means I obtained two tinctures, differing slightly from each other, although in appearance very much alike. It is to be observed, however, that the first remained perfectly transparent after dilution, while the other became turbid; that the brownish precipitate produced in it by ammonia or potash was much more flocculent, having at first a tendency to collect at the top of the liquid; and lastly, that this tincture did not turn black with infusion of galls, which the other did. This last circumstance, together with the method of its preparation, led me to surmise that, notwithstanding its red colour,

and solubility in alcohol, and permanence in the air, it might possibly be a protosalt. To determine whether this was the case I tested it with the red and yellow prussiates of potash; when what was my astonishment, to discover that these reagents produced not the slightest change in it. I found a similar indifference towards these tests in the other tinctures. This circumstance certainly very much surprized me: it proved that the usual tests were insufficient to determine the true composition; that the tincture No. 2 contained peroxide, was rendered evident by its behaviour with tincture of galls, but this made the solution No. 1 only the more difficult to understand. Could it be that Dr. Barker's view was correct? and that it was a combination of proto-acetate of iron, with acetate of potash? the proportions employed would appear to indicate otherwise. Using equal weights, the quantity of acetate of potash was rather under than above what was necessary for a perfect exchange; so that if the deposit consisted simply of sulphate of potash, there could not have been any surplus acetate to have combined with the salt of iron: to prove whether this was the case, I collected some of the sediment, and having digested alcohol on it, till it poured off quite colourless, I diffused the remainder through water, which it rendered brownish and turbid, but became perfectly transparent upon adding some drops of sulphuric acid. This solution threw down a copious brown precipitate, by the addition of caustic potash. It was evident that a quantity of iron remained undissolved, it might be as a double sulphate; and the probability of Dr. Barker's theory being correct was therefore very much increased.

These experiments induced me to prepare this substance, under circumstances where no fallacy would be likely to operate; and I determined to ascertain, by the direct mixture of the constituents, whether such a combination really existed as Dr. Barker supposed, and if so, what were its properties. For this purpose I prepared a solution of proto-acetate of iron, by a

method similar to that recommended by Dumas.* I decompounded acetate of lead by proto-sulphate of iron, in a phial completely filled with rectified spirit, and well corked; and the sulphate of lead having completely subsided, I decanted the supernatant liquid. This was clear and colourless when confined, but upon exposure to air, almost immediately threw down a brownish sediment of sub-peracetate of iron. This solution obeyed all the re-agents for a protosalt of iron; precipitating white, rapidly changing to green, with carbonate of potash; white changing to blue with yellow prussiate; and deep blue with red prussiate of potash.

I next dissolved some acetate of potash in rectified spirit, and added it to the spirituous solution of proto-acetate of iron, when instantly the mixed liquids, from being colourless, turned of a deep claret colour. Delighted with this evidence of combination, I next examined the properties of the mixture: exposed to the air it suffered no alteration; evaporated to dryness it afforded a tough, brownish, deliquescent mass, which dissolved again without change; it threw down a reddish-brown, flocculent precipitate with the caustic alkalies; but, what was most surprising, it underwent no alteration with red or yellow prussiates, or tincture of galls. Now it is perfectly evident that this is an instance of chemical combination; we find a complete alteration both in the physical and chemical properties of the proto-acetate of iron; it acquires a red colour, is unaltered by exposure to air, and is no longer acted upon by re-agents. It will also be observed, the exact analogy which this mixture presents in all its characters to the tincture No. 1; and it cannot be considered presumptuous to assume, that these two preparations are identical in composition.

It becomes a very interesting subject of inquiry, in what manner does combination with acetate of potash render the protosalt of iron permanent in the atmosphere? We can

* Traite de Chimie appliquée aux Arts. Tom. v. p. 161.

explain it, by supposing that the positive potash, charging the iron negatively by induction, diminishes its affinity for oxygen; and this explanation perfectly accords with a number of interesting phenomena observable in the mineral kingdom. I lately had occasion to analyze the water of a mineral spring, which I found to contain proto-chloride of iron, along with the chlorides of some earthy metals.* Again, it has often surprized me, the existence of rhomboidal carbonate of iron, perfectly white and protoxidized; but we are to recollect that this salt is isomorphous with carbonate of lime, and consequently frequently, if not always, contains some of it.

A similar explanation will account for the absence of the usual behaviour with the ferro-cyanurets of potassium; the protoxide of iron being here in the capacity of an acid, has lost the tendency to exchange principles with the basic cyanuret of potassium, which it usually possesses in its positive state.

Being thus led to the determination of the true composition of this remarkable preparation, my next object was to ascertain the causes of its frequent failure, and I must acknowledge as my principal clue to the understanding of this phenomenon, an experiment detailed to me by Dr. Leet, and repeated by myself. He rubbed up two parts of acetate of potash with one of sulphate of iron, dissolved completely in water, and then added about twice the quantity of rectified spirit. When the sediment had completely subsided, a perfectly limpid, colourless liquid remained supernatant; now the question was, whether the presence of the water or the precipitation of the sulphate of potash by alcohol removed the colour.

To ascertain this, I added a quantity of water to the double acetate of protoxide of iron and potash, and after a considerable time no change was produced, greater than could be accounted for by mere dilution. I then added to another very

* A similar analysis is in, I think, the July Number of the Records of Science.

deep-coloured solution of the same salt in alcohol, a solution of sulphate of potash, when, after a few days' subsidence, the supernatant liquid became perfectly colourless. Upon exposing this liquid to the air, a brownish precipitate rapidly formed; the tests for protoxide of iron acted characteristically. In fact, it was plain that the precipitating sulphate of potash carried down along with it the acetate of the same base, leaving the uncombined proto-acetate of iron in solution.

Thus we have explained to us the necessity for using strong alcohol, of decanting as soon as the sediment has fully formed,—practical precautions long known to manufacturers, but until now unaccounted for.

It only remains at present to recapitulate the results to which these investigations lead. It is evident that, according to the method employed, there are two very different compounds. That the tincture No. 1 offers very great advantages to the physician, presenting to him a protosalt of iron, unchanged by the air, and compatible with astringent infusions. That its failure depends on the precipitating sulphate of potash separating the acetate of the same base from the tincture, and thus rendering the salt of iron liable to speedy oxidation. I am at present engaged in researches connected with the other preparations of iron, which promise exceedingly interesting results.

Art. XVII.—*Determination of the Question, which are the Nerves of Taste.* By B. Alcock, M.B.

[Communicated in part to the British Association at Bristol, August, 1836.]

My attention having been particularly directed to the physiology of the fifth nerve, the uncertainty in which the relations of the sense of taste have remained, even to the present time, has led me into a series of investigations, the results of which, I hope, will be considered worthy of being submitted to the profession.

The first question relating to the sense which engaged my attention, was the determination of the doctrine of Panizza, that the glosso-pharyngeal nerves are the special nerves of taste. His investigations have been extended to the faculties of the tongue in general, and the functions of its several nerves; and from a series of experiments, which consisted in the division, during life, of the ninth nerves, the lingual branches of the fifth, and the glosso-pharyngeal nerves, severally, in animals, upon both sides, he has arrived at conclusions, of which those relating to the sense of taste, and the nerves supposed to be connected with it, are the following. It is to be premised, that he decided the presence or absence of the sense by the test of coloquintida, a simple bitter, but so intense that few animals, possessed of taste to a degree at all acute, disregard it; while it is so devoid of smell, that they are not prevented thereby from taking food embittered with it. His conclusions are, first, that the division of the lingual branches of the fifth nerves is followed by complete extinction of the tactile sensibility of the tongue, while motion and taste remain: secondly, that division of the glosso-pharyngeal nerves is followed by loss of taste alone, motion and tactile sensibility remaining; and thirdly, that the glosso-pharyngeal nerves neither are sensitive, nor possess any influence over muscular contractility. Those conclusions, it will be observed, are altogether at variance with the opinions generally entertained upon the functions and properties of the nerves in question; his first, indeed, agrees with them so far as the tactile sensation of the tongue is concerned, but in a most important particular it differs altogether, viz. as regards taste. According to others, the division of the lingual branches of the fifth is followed by loss of that function in the tongue, while according to Panizza it continues unimpaired. His second is still more opposed to existing doctrines; and with regard to it, he must, I believe, be conceded the claim of originality; for though the nerves in question have been considered at some time to take part in the sense, he is the first who has pronounced the glosso-pharyngeals to be the special and sole

nerves of taste. In his third, again, he differs equally from the received opinion; according to which the glosso-pharyngeals are nerves of muscular motion and ordinary sensibility to the pharynx and root of the tongue. Such is the conclusion of Mayo, who has inferred their function from the effects produced by irritating the nerves after death, in animals recently killed, and from their distribution. Panizza, on the contrary, asserts, that "if the nerve be pinched or punctured before it be divided, neither does the animal give sign of uneasiness, nor is the tongue convulsed;" and the following is his final account of the effects of the division. "Let it suffice for all, that the dog in which the glosso-pharyngeal pair was divided, lapped water and eat as freely as if he had suffered no injury, and that afterward mastication and deglutition were perfect; but he had no other guide than smell in the choice of his food, so that he swallowed with the same readiness the most disgusting and the most noxious, and the most agreeable and beneficial articles, provided either they did not smell, or their odour was artificially disguised or blended with another, agreeable to the animal. In my experiments, the dog eat with equal avidity fresh animal food, or that sprinkled with coloquintida, and drank milk or pure water as willingly as that rendered bitter by the same substance."*

The determination of those views appeared to me the more desirable since they have been adopted by an authority, whose extended researches into the functions of the nerves entitle his opinion to much consideration; I mean Mr. Broughton; who, in the Edinburgh Medical and Surgical Journal of April last thus writes: "but Panizza's experiments with the coloquintida shew this suggestion to be erroneously formed; and leave no doubt of this nerve—the glosso-pharyngeal—being the medium of the impressions of taste." He has also brought comparative anatomy to the confirmation of this opinion: thus; "it appears then that the problem of the medium of taste is now solved;

* Ed. Med. Surg. Journal, Jan. 1, 1836.

and by a simple appeal to experimental physiology. And it is doubly satisfactory to find that this successful appeal is confirmed by analogy and comparative anatomy. Mr. Owen, of the College of Surgeons' Museum, whose knowledge of comparative anatomy is well known, observed, that he never could discover in the tongues of birds any nerve corresponding with the *lingual* or *gustatory,* as it has been erroneously called. But he always observed that the *glosso-pharyngeal* nerve was freely distributed and terminated in the soft papillary substance of the tongue."* Further, I have been informed, that a communication was made to the British Association at Bristol, from Dr. M. Hall and Mr. Broughton; in which they professed to have repeated and confirmed the experiments of Panizza. The name of Panizza is so eminent, and his statement so circumstantial, and stamped with candour, that one must hesitate to call his opinion into question: however, it is so opposed to received doctrines that I determined to repeat the experiments upon which it has been founded, and I shall now submit the results.

Before doing so it is but right to premise, that Mayo has made the following objections to the conclusions of Panizza with regard to taste: viz. that the continuance of taste after the division of the lingual branches of the fifth, may be due to the soft palate; that when the sapid substance is applied to the tongue alone after their section, either it becomes diffused, or the position of the dog makes him uneasy; that after the division of the glosso-pharyngeals, the animal ought still to have tasted by means of the soft palate, and that the sense of taste is most acute at the tip and edges of the tongue; whereas, according to Panizza, the base of the tongue is most sensible to taste. Those objections are certainly most forcible, and so far as mere reasoning can do it, they fully meet the Professor's views; but after all, demonstrative evidence is wanted, which can be obtained only by a repetition of the experiments, and is not furnished by Mayo: he, it is true, divided the glosso-pharyngeal nerve on one side,

* Ed. Med. Surg. Journal, April 1, 1836.

and the lingual branch of the fifth on the other, and the animal tasted; but manifestly such an experiment cannot decide the question.

The following are the results, which I have obtained from the division of the lingual branches of the fifth, and of the glosso-pharyngeals; 1st, after the section of the former the animal lost the tactile sensibility of the tongue, and retained the sense of taste; but, though it tasted, it appeared evidently to be by other parts of the mouth, and it seemed equally plain that it had lost the sense in the anterior part, of the tongue, at least; for after it had been trained to suffer its mouth to be opened, quietly, and to have something laid upon its tongue, it allowed the pure coloquintida, moistened, to remain so long upon that part of it, care being taken to confine it to it, that no doubt could remain that the animal did not taste by the organ at its anterior part; but as soon as it was let go and closed its mouth, it displayed at once its sense of the taste by the ordinary indications. The inference therefore of Panizza, that taste continues *unimpaired* in the tongue after the section of the lingual branches of the fifth, is in my opinion incorrect; so far at least as it is understood in an unqualified sense, and its application extended to all parts of the organ. 2nd. I have divided both glosso-pharyngeals in several dogs, with the following results: in two of them, each time that either nerve was taken upon a hook, preparatory to its division, a most violent action of the pharyngeal muscles was excited, resembling precisely the pharyngeal effort to reject the matter, which most persons must have experienced, when something nauseous or revolting has got beyond the power of the tongue, and entered the pharynx; and it was so imperative, that the animal struggled to get upon its feet, and was with difficulty restrained. In a third, a similar effect, only less marked in degree, was produced, by pulling upon the nerve with a ligature passed underneath it; and in one of the three the root of the tongue was at the same time depressed, and rendered concave from side to side. In three of the animals, muscular startings occurred in the throat, while the

nerve was held upon the hook; but the phenomenon just described did not, at that time, take place. In one of those the startings were strong and repeated; in the others, though obvious, they were less remarkable; and in one also, as has been mentioned, the first effect described was caused by pulling upon the nerve. In all but one, the animal displayed much suffering from stretching, pinching, and division of the nerve. In every instance deglutition was more or less impaired; I say more or less, because in my first attempts I did not succeed perfectly in dividing the nerve, but only its pharyngeal division; and this occurred in the animal which I presented to the Surgical Society of Ireland, during the past session; as I afterward learned from dissection: in that instance the animal required at times repeated efforts in order to get a morsel down; though at others it appeared to swallow with tolerable freedom. But in those instances in which the nerves were perfectly divided, deglutition was so very much interfered with, as in some cases to be impossible: the animal took, and masticated the morsel as usual, and swallowed it from the mouth into the pharynx freely; but then, after repeated and unavailing efforts to get it further, it made equal efforts to get it back again, in which it often experienced so much difficulty, as to become much exhausted, or to seem even in danger of suffocation, before it succeeded. Yet even here the animals, under certain circumstances, were able to get food down—meat in small bits and moist—but if either in large bits or dry, it would not pass. Panizza's conclusion, therefore, with regard to the influence of the nerve upon deglutition, as also that with regard to its sensibility, appears to me as incorrect as the former.

The power of the animal still to swallow, may be readily explained by reference to the mechanism of deglutition; which process consists not of a single act, but is accomplished by a succession of actions in the several successive portions of the part of the intestinal tract engaged in it: hence, where no difficulty exists, the loss of action in one part, the pharynx, may be compensated

by the action of the part immediately behind it—the mouth—which may suffice to propel a morsel through the former into the œsophagus, and this effect will be favoured by the natural tendency of the matter to descend; but where, from any cause, the pharynx is called on itself to act; then difficulty arises.

Lastly, the animal's taste did not appear, to say the least, much affected: even immediately after the operation, when exhaustion renders it in general very indifferent, it unquestionably tasted the coloquintida introduced into its mouth,—in one to such a degree, that efforts to vomit were excited by it,—and when food embittered with it was offered to the animal, it rejected it, though it appeared to me to manifest less aversion than previously. In those experiments as much, as could be, of the nerve was in each removed, to provide against reunion. The inference from them, as regards taste, is obvious; that the glosso-pharyngeal nerves, if connected at all with the sense, are not the sole or special media of its perception.

On the other hand, if they be not the special nerves of it, how are we to explain the existence of taste in the bird; in which, it is, as has been stated, asserted that no nerve corresponding with the lingual or gustatory branch of the fifth can be discovered; if birds taste by their tongue, it must be inferred from the asserted anatomy of their nerves, that the glosso-pharyngeals are with them media of taste. This being an interesting question, I have directed particular attention to it, and I have, I think, succeeded in removing the objection. In the first place it is to be remarked, that it is by no means certain that all birds taste with the tongue; it is even maintained—though the position seems to me untenable—that with them the tongue is only an organ of prehension; it is therefore possible for them to taste through the fifth nerve, by means of their palate, though they should not have a lingual branch: in the second, though a lingual branch of the fifth may not exist in

* I have seen the dog, under such circumstances, eat a little and refuse more.

some, or its existence be a matter of doubt, yet I have ascertained most satisfactorily, that it does exist in others, which do positively taste by the tongue; for instance, in parrots. It is true that birds do not, any of them, so far as I know, possess a lingual branch similar in all respects to that in the mammalia; but it will be found on more particular inquiry than the point appears to have received, that they are provided with one corresponding thereto. It exists, I believe, in all, but is not equally developed: it is not, however, as in the mammalia, an original division of the third branch of the fifth, distinct from the inferior maxillary; but it is a branch of the latter given off from it immediately after it has entered the maxillary canal; it is transmitted downward through the jaw, and escapes from it inferiorly upon its inner aspect; it then runs forward between the jaw and the tongue, giving filaments as it proceeds: in the other birds in which I have sought it, I have not succeeded as yet in tracing it satisfactorily beyond the frænum; but in some of the parrot tribe, in which it was proportionally much larger than in other birds, I have found it continued along the inferior lateral aspect of the tongue to its extremity. The disposition of this branch obviously removes the difficulty, and reconciles the gustatory office of the fifth to the anatomy of that nerve in the bird. It is, however, to be at the same time remarked, that this lingual branch of the bird bears but an inconsiderable proportion to the lingual portion of the glosso-pharyngeal nerve in the same animal; and this particular is still more remarkable in the parrot tribe, in which the lingual branch of the fifth seems most developed, than in some others; in these, so far as my experience at present extends, the lingual branch of the glosso-pharyngeal is single; runs along the upper surface of the tongue; distributes branches as it proceeds; and is smaller, still more so comparatively, than either of those in the parrot tribe: in it the glosso-pharyngeal gives two lingual branches of considerable size; of which one runs along the upper, the other along the under aspect of the tongue; they give off few, if any, filaments during their course; they are pro-

longed to the extremity of the organ; and they there break up into a number of filaments; which enter the structure, which appears to be, in those birds, the chief, if not especial seat of taste.

The first of the results stated to have been produced in my experiments upon the glosso-pharyngeal nerves claims particular notice: even now, I do not feel perfectly assured as to its cause, which might be regarded either the direct influence of the nerve over the actions of the pharyngeal muscles; or an indirect one exerted through its connexion with the sensation of the surface upon which the nerve is distributed: either of which will suffice to explain the phenomenon. The first does not require to be dwelt upon, as its adequacy is manifest. But it appears to me that the effect produced does not coincide strictly with that usually consequent upon what is considered the direct influence of a nerve upon muscular action; inasmuch as the latter amounts to no more than a rapid, irregular, and repeated contraction, arising sometimes to convulsion, of the particular muscles which the nerve supplies; but not to a modulated, associated action, such as occurred in the case under consideration; which strictly resembled that ordinarily excited by a disagreeable sensation experienced in the fauces or pharynx; and in which other muscles were engaged as well as those supplied by the nerve. On the other hand, it is well known that impressions, made upon nerves of sensation at points intermediate to their cerebral connexions, and their surfaces of distribution, are ordinarily referred to the latter; and thence the irritation of the nerve was likely to have caused an uneasy sensation in the passages alluded to; the natural effect of which is such an effort to discharge the offending agent—supposed in the present case—as was excited by taking the nerve upon a hook: and again, I have had occasion more than once to observe an analogous effect caused by taking the lingual branch of the fifth, which we regard as merely sentient, upon a hook; on doing which, the animal has frequently made such motions with the mouth, as would be excited by the presence of something therein, or by

the perception of an impression upon the tongue. I am for those reasons disposed to regard the result in question as the effect of a sentient impression, excited through the nerve and referred to the interior of the pharynx; and the production, in other instances, of mere muscular startings by the irritation of the nerve, tends to confirm this view. I must, however, add, that the circumstance may probably be as well explained by an exalted degree of muscular excitement, or by a higher one than that necessary to produce the simple starting: but to the latter view the two circumstances adverted to; viz. the engagement of other muscles, beside those supplied by the nerve, and the associated action; are very strong objections.

How then are the results of Panizza's experiments to be explained? This task belongs properly to another; but justice to my own induces me to suggest two sources of fallacy, by which he may have been misled. I cannot, in the first place, believe that he operated on the glosso-pharyngeal nerve; and failed to find it sensitive, or to influence muscular action; yet he asserts that it is devoid of both properties; or that he divided the nerve completely, and that afterwards deglutition was perfect. Indeed, from my own experience of the operation, I should say that no one; who makes the experiment once only, or for the first time; is likely to succeed in his purpose; inasmuch as the lingual division of the nerve is so close to the base of the cranium; is so short; and runs so directly inward away from the pharyngeal, that it is almost certain to be overlooked, until, after having, as he thought, cut the nerve at its exit, the operator finds on dissection *post mortem*, that he has left the lingual portion untouched; and from a comparison of my experience with the history of the operation given by Panizza, I can hardly hesitate to assert that he did not reach that portion; else he would not have omitted to mention a source of failure the most remarkable in the case: again, he makes no reference to the external carotid artery, which is situate between the surface and the track of the nerve, and which it is, in my opinion, ne-

cessary to tie with a double ligature, divide and displace in order to allow sufficient room, and to guard against hemorrhage. If not the glosso-pharyngeal then, what other nerve could it have been? It might be the superior laryngeal: indeed this nerve is situate so near to the other, that it has very frequently presented in the course of my operations; and I have been for some time uncertain which of the two it was: it is doubtless somewhat lower and somewhat more superficial than the glosso-pharyngeal; but it will be readily understood, that with a deep wound, and a trying operation, the difference in both respects might be easily overlooked. Again, I have not found, in the trials I have made, the superior laryngeal display either of the disputed properties; and, if this conjecture be correct, we can explain how he may have divided, as he thought, the former nerve; and found it devoid both of sensibility, and of muscular influence. In the second, beside the acknowledged power of hunger, I have found that some dogs are naturally so devoid of taste, that a conclusion could not be safely drawn with regard to that sense, from the result of any experiment upon them; and I have actually rejected a dog as an unfit subject on that account: the animal ate—uninjured—with avidity, food, which others—naturally voracious—rejected with disgust: and taste might have easily seemed obliterated in it after the division of the glosso-pharyngeals.

It being then, as it appears to me, satisfactorily proved by the preceding details, that the glosso-pharyngeal nerves are not the special media of taste, we are thrown back again upon the fifth nerves, to which it has been generally attributed. But if *they* be the media, with which of their branches is it associated? or, is it restricted to particular branches? The linguals are those which have been generally considered the nerves of the sense; whence they have obtained the name of "gustatory." Several considerations, however, cast a doubt upon that opinion: first, if they be nerves of taste, they certainly are not the sole; since it continues after their division; and secondly, if taste be

a faculty derived from them, it might be expected that all parts of the mouth supplied by them should possess it; but they give filaments to parts, which are not seats of taste; and thus, while they are proved by the first not to be the sole, the second throws a doubt upon their being nerves of taste at all. Again, in the section of the lingual branch of the fifth, that of the chorda tympani is necessarily involved, and the effect consequent on the division of the former may be due to that of the latter; while it is one of a set, and the only set, of nerves distributed from a common source to the seats of taste in the human subject, viz. the branches of the spheno-palatine ganglion. Those considerations; to which is to be added that suggested by Mr. Noble, that no other function has been with any degree of certainty assigned to the chord; appeared to me so forcible as to induce me to adopt the opinion, that the ganglion and its branches are the source and media of taste to the tongue, and fauces, the seats of the sense in the human subject at least. The same opinion has been already suggested by Mr. Noble of Manchester, and advocated by him on rational grounds. Bellingeri also maintains the doctrine that the chorda tympani is the source of taste; and it appears further to have been adopted by Majendie; inasmuch as we have been lately informed of his removing a defect of taste by passing a galvanic current through the course of the chord. To the doctrine two objections, however, present themselves; viz. that the branches of the spheno-palatine ganglion are distributed to other parts beside the seats of taste—the nose—and that even its palatine branches cannot all be considered nerves of the sense, since we do not taste with the hard palate; and hence either taste is not a special sensation, or those are not its special nerves. On the other hand, those objections might be removed by the principle, too much disregarded in our reasonings upon the source of sense, that peculiarity of sense may be equally, or even more, the result of peculiarity of organization in the organ, as of peculiar nerves; and experiment alone offered a prospect of solving the difficulty.

But how experiment upon the spheno-palatine ganglion and its branches? Notwithstanding the apparent impracticability of it, it occurred to me to attempt the removal of the ganglion from the dog, and thus directly decide the question. In this design I was encountered at the outset by the authority of Majendie and Desmoulins, according to whom neither ganglion nor chorda tympani exist in that animal. On dissection, however, I ascertained with great facility that both do exist, and not only in the dog, but also in every one of the individual mammalia from which they expressly state that the parts are absent. Further I learned that the dog presents a most felicitous opportunity for determining not only the influence of the ganglion upon taste, but also its relation to the fifth nerve. In the human subject, the ganglion exists upon a branch of the nerve; and hence it has been, for the most part, assumed and described to be a part of the fifth; but in the dog it does not engage the nerve; it is separate, and is only attached by its anterior extremity to the naso-palatine branch of the second division; while its posterior gives off, or receives, as it may be, the chorda tympani: its characters, too, are altogether different from those of the branches of the fifth. It is thence manifest that it cannot be regarded as an essential, original, or necessary part of the fifth, but only an adjunct, and pretty certainly a part of the sympathetic system; with the main portion of which it is connected by one of its posterior filaments.

After several attempts I succeeded—and for the accomplishment of the experiments which I am about to relate, I take this opportunity to make my acknowledgments to the two friends who assisted me; Mr. Williams, Lecturer on Chemistry, and Mr. Thomas Hart, Conservator of the Museum in the Medical School, Park-street; without whose patient, cordial, and judicious co-operation, as well in them as in my other experiments, my attempts must have failed—I succeeded in the removal of the ganglion from both sides. In order to do so it is necessary to open the zygomatic fossa, by displacing the

zygomatic arch, and the coronoid process of the lower jaw, with the insertion of the temporal muscle; that done, the superior maxillary nerve may be exposed, lying upon the pterygoid muscle; and on drawing aside or raising the nerve, the ganglion will be found beneath it upon the muscle: the deficiency of the wall of the orbit, and the consequent size of the fossa afford abundant room for accomplishing satisfactorily the several steps of the operation.

I have thus twice extirpated the ganglion from both sides; and in both instances the animals tasted acutely afterward. We must then abandon, as groundless, the idea that taste depends, whether for its existence or perception, upon the presence of the ganglion or the chorda tympani, and seek another office for those parts.

The conclusion, that taste is independent of them, is confirmed by the existence of the sense, to a very high degree, in animals, in which neither ganglion nor chord has been discovered; for instance, birds; and if those parts have any connexion with the sense, it must be by some other relation than that of the media of perception. That they have a connexion I am strongly disposed to think; because in the animals in which they exist, they are connected especially with those nerves, which will presently appear to be at all events the chief media of perception to the sense; and because, where they are absent, the seats of the sense are probably less numerous, and the apparatus more simple. It is necessary to suppose the existence of some superadded provision; else how is the sympathetic action of the several parts of the apparatus, for instance, the rapid flow of saliva from the impression of a sapid agent, to be explained; but at present this is but speculation, and premature.

It next occurred to me, that taste, whatever be its real nature, may be a sensation perceived through the ordinary nerves of the parts where it resides, viz. the palatine and lingual branches of the fifth; and that the question might probably be decided by the section of those branches. For this purpose, the dog

also offered a favourable subject; inasmuch as an interval of some lines exists between the origin of the naso-palatine nerve, and the attachment to it of the extremity of the ganglion; and thus an opportunity was afforded of cutting the nerve between those points, without interfering with the ganglion or its connexion. I performed the experiment in two modes: in two dogs I simply cut the lingual branches below the jaw, and the palatines at the interval described, by which proceeding the influence of the ganglion over the latter was not interrupted; and in two others, at the same time that I cut the palatines, I also removed the ganglion; and in both cases the result was *quam proxime* the same, viz., that taste was very nearly obliterated, though sufficient evidence of its presence was still manifested to compel me to admit its existence: the effect produced was, that the animals, which had previously displayed the usual disgust at the coloquintida, no longer manifested any; but only ceased to eat food embittered with it. They would eat several bits in succession, and then refuse to take more, or drop the bit, taken, from the mouth, and take presently a clean bit if offered to them.

I examined two of those animals—one of each—after death; and I found the nerves so fairly divided, that the experiments appear to me unexceptionable. I must also add, that they were accomplished without injury to any other nerve than those mentioned, with the exception of the masseteric branch of the fifth, and part of the superficial temporal, which were of necessity divided; but neither the buccal branch, the infra-orbital nerve, nor the portio dura was injured. All positive conclusion from them must therefore apply to the palatine and lingual branches of the fifth. They confirm, in the first place, the conclusion already drawn with regard to the ganglion and the chord. In the second, they prove positively that the palatine and lingual branches are, in the mammalia, media, and probably the principal media of taste; so much so indeed, that in the first instance, in which the experiment was made after the first method, and in which the palatines were divided on both sides, within

the same day, the dog seemed absolutely indifferent to a strong solution of the coloquintida poured in quantity into the mouth and fauces; but the animal had received a severe shock, and that the *absolute* indifference to the bitter was due to the shock, appears to me satisfactorily shewn by the result of the experiment when differently conducted, *i. e.* when an interval of several days was allowed to intervene between the two operations; in which case the effect was such as has been detailed. It should be known, in order to complete the latter inference, that as, on the one hand, when the lingual branches alone are cut, taste seems unimpaired; so on the other, the same result is obtained, when the palatines are the nerves divided; a fact which I have had an opportunity of witnessing in the course of the preceding experiments. In the third place, they show, that it is as branches of the fifth nerve itself, and not in virtue of their connexion with the spheno-palatine ganglion, that the palatines act as media of taste; and lastly, they indicate, in the subjects of experiment at least, another medium.

For the performance of this office, two sets of nerves offer; either the other branches of the fifth, distributed to the mouth, or the glosso-pharyngeals. There does not appear reason to suppose that the former take part in the function; because, first, in the human subject, in which alone we can hope to be able to decide the point, taste is certainly not enjoyed by other parts of the mouth than those already mentioned; and therefore other branches of the fifth than the palatine and lingual are not likely to be connected with it; and secondly, because in some of the experiments upon those, the buccal branches also have been divided, and the result was the same. There remain then only the maxillary branches, and the filaments of the lingual not distributed to the tongue, and, they being likewise consumed in parts devoid of taste, it is to be inferred that they also are not associated with it. On the other hand, the manner in which the glosso-pharyngeal nerves are distributed in all animals; but more particularly in birds, and especially in parrots; can leave no

doubt that they fulfil in all a function of sensation; and as well the manner of distribution, as the disproportion between them and the lingual branches of the fifth in the parrot, which manifestly tastes by the tongue, would lead to the inference, that in that bird the glosso-pharyngeals are the chief, perhaps the sole nerves of taste to that organ; and here again experiment alone can decide. This might probably be accomplished upon the parrot; but I have not had an opportunity of attempting it. I have, however, succeeded, I think, in determining the point upon the dog. In one, upon which I had before divided the palatine and lingual branches of the fifth, on both sides, with the effect already described, I afterward cut the glosso-pharyngeals also; and I believe, so far as a single experiment can decide the question, that the animal was then absolutely tasteless. On the fourth day after the operation, I tested him with the coloquintida twice within a few hours, and each time he eat without hesitation a meal of meat containing so much of the bitter, that I fear it caused his death: at all events he sickened, an hour or two after the second meal, and did not recover.

My experiments, therefore, justify the inference; on the one hand, that the sensation of taste is perceived through more nerves than one, and on the other, that the glosso-pharyngeals, though not the sole or special media of its perception, have yet a part in the function, and that they are nerves of taste as well as the lingual and palatine branches of the fifth; and if this conclusion be confirmed, the difficulty furnished by the disposition of the fifth nerve in inferior animals, as an objection to its gustatory functions, is still further removed; and we may feel at liberty to estimate the relative gustatory properties of the two nerves according to their relative development and distribution. The result just stated, further, furnishes a possible solution of those obtained by Panizza, supposing him to have operated on the glosso-pharyngeal nerves; inasmuch as were the faculty, as exerted through the fifth, weaker, and that through the other nerves more active in the subjects of experiment; the result might be easily explained; but I must repeat that I incline

to the opinion, that he either did not operate on those nerves, or that, if he did, he divided only their pharyngeal portions.

It may be objected, that the perception of taste through two nerves is inconsistent with its being a special sense. To this it may be replied, that the special character of the sense is not universally admitted, and that, though it were, the inconsistency remains still to be proved. It has been usual to regard a special nerve as a necessary part of a special sense; but while certain anatomical considerations, upon which I cannot at present enter, cast doubt upon the necessity of such a condition, the difficulty of conceiving the perception of a special sensation through a two-fold medium, exists only while an instance of it cannot be found; and it appears to me, that the instance is furnished in the case under consideration.

But is taste a special sense? Few questions probably remain in a less settled state than this: few also present greater impediments to grappling with the point in dispute, because of the several complications by which it is beset and obscured. The tests by which I would determine it are two; first, the existence of an especial sensation; secondly, the existence of a special organization; and until those be answered, the special nature of the sense in question must remain undetermined. The apparent difficulty in the case of taste is enhanced by the facility of the application of the tests to the other senses; thus the distinct and independent nature of sensations of sight, smell, hearing, and touch, as well as the existence of peculiar and appropriate organs connected with such sensations, is manifest; but it is not equally so with those of taste; in which, for the most part, the particular impression is, and seems necessarily, associated with sensations of touch. Hence it has been inferred, that taste is only a modification of the latter, and that its sensations probably amount to no more than very fine impressions of touch.

The conclusion stated appears to me for many reasons untenable; for, in the first place, if agents properly sapid be applied in a solid form to the tongue, it will be found that their tactile

and sapid impressions are not cotemporaneous, but the sensation of touch will be experienced at once, and that of taste not until solution of the substance has taken place ; nay, we may even have become insensible to the contact of the matter by the time the taste begins to be felt ; and those facts can hardly be reconciled with the opinion, that the two sensations are of the same kind. In the second, if taste be only a degree of touch, it is difficult to explain why it should not reside, in proportion, in one part, endowed with tactile sensibility, as well as in another? why it should be restricted to one and not enjoyed by all parts, at least of its own proper seat? Now truly sapid agents excite their characteristic sensation only in the mouth and fauces, and their influence is not even extended over those regions universally, but confined to particular parts of them ; while, on the contrary, those agents, which are either simply objects of touch or simply irritating, affect all parts of the mouth and other surfaces beside those of the mouth in a similar manner, and in a degree proportioned to the delicacy or sensibility of the particular part, to which they may be applied. Thus, bitter substances, which are selected as unquestionable instances of sapid agents, do not, if simple, produce any sensation but that of contact, when applied to the conjunctiva, or any other surface than that of the mouth ; while irritating agents produce their effect, to whatever surface they may be applied ; whether that of the mouth, the eye, the nostril, or even the common integument, provided the epidermis be sufficiently delicate : again, the bitter does not excite its sensation in all parts of the mouth ; it does not affect the inside of the lips, that of the cheeks, the hard palate, the under surface of the tongue, or the gums, and its impression ceases when it has descended a little way into the pharynx. Here then is a manifest ground for distinction between sensations of taste, and those either of touch or of general sensibility. But, it may be said, that the facts adverted to are compatible with the opinion, that taste depends simply upon a greater degree of delicacy in the other senses resident in the parts, to which it is referred ; which

being more obtuse in other parts of the same surfaces, the sensation is not excited by the impression of sapid agents; this, however, is but a *petitio*, and is altogether at variance with the comparative sensibility in other respects of the several parts of the mucous surface; for, if so, the most sensitiveshould be the seats of the sense. Now, the sensibility of those, where it does reside, is inconsiderable, when compared with that of others, where it does not; thus the sensibility of the conjunctiva, and of the Schneiderian membrane in other respects far exceeds that of the seats of taste; and they are not sensible to the impression of sapid agents; and in like manner, though the tongue possess a high degree of tactile sensation, it does not surpass in that respect the extremities of the fingers, which, on the other hand, far exceed, in tactile sensibility, the soft palate. Lastly, a perception of contact is not necessary to taste at all, any more than to smell, sight, or hearing, as may be at once shewn by the inhalation, whether through the mouth or nostril, of many sapid agents; for instance, coloquintida or aloes, in an impalpable form, and of which the inconvenience experienced in powdering those substances furnishes a striking example.

Those several reasons seem to me quite sufficient to prove, that taste is a sensation altogether distinct from, though for the most part associated with, touch. The special character of taste is also disputed, because the flavour of many substances, which is usually accounted a sapid impression, may be traced to another source: thus it is said that, "first, when the mucous membrane of the nostrils is inflamed, the flavour even of sweet, bitter, or acid substances is not perceived, although there be no symptoms of disease in the mouth, and all the other sensations felt there are natural. Secondly, by carefully inspiring during the whole time when a sapid substance is held in the mouth, or by carefully expiring through the mouth only, its flavour may be rendered imperceptible, even though it be rubbed repeatedly between the tongue and palate; a sense of what is usually called irritation or pungency, more or less acute, according to the nature of the substance in the mouth, only remaining; but on the

slightest expiration through the nostrils the true flavour becomes immediately distinct:" and hence the author has inferred, that flavour "is, in fact, the sensation of smell produced by effluvia, from the substance held in the mouth, passing through the nostrils during expiration."*

That flavour is, in many cases, only an impression upon the sense of smell, referred for certain reasons to that of taste, is undeniable; but that it is not so in every instance, appears to me equally certain, and will be evident from a careful examination of the data upon which the contrary has been assumed.

Before attempting such examination, it is necessary, for the understanding of the point in dispute, to premise that all agents which produce an impression in the mouth, and thereby excite sensation, are not to be accounted sapid: that some produce an impression of touch; some a chemical or irritating impression; and some again a sapid impression. Now those three modes are easily established, and are by no means to be confounded: it will be readily admitted that the sensations excited by objects of touch and by irritating or pungent agents are different; thus, that produced by a portion of rock crystal taken into the mouth, and that of croton oil, or a simple acid, when applied to the tongue are not the same; neither are they sensations of taste, as will presently appear more fully, nor can they by any modification become such, except by contributing to create a complex one; which is still in strictness no more a sensation of taste, than of any other of the simple sensations composing it; and the elements of which are distinct and distinguishable. Again, it is to be borne in mind, that, in consequence of the communication between the mouth and nose, substances endowed with properties capable of acting both on the nose and mouth may, when taken into the latter, produce, according to the number of properties possessed, a variety of sensations, affecting both organs at the same time. Should it, in such case, happen that

* Alison's Physiology.

the substance is capable of exciting a sapid sensation in the mouth, as well as an impression upon the organ of smell, the former will be modified by the latter; or, which is more in point, should it be capable of exciting a sensation of smell, without any other impression upon the mouth than that of contact; and there may be found many such substances; the sensation of smell will be referred to the mouth, in consequence of the mode in which it has been obtained, and that will be accounted a sensation of taste, which is in reality one of smell. If in such case the faculty of smell be by any means suspended, the ordinary impression of the substance will be also suspended, and thus the sense of taste seem obliterated by the interruption of that of smell.

Let us now return from the preceding digression to the reasons for asserting flavour to be only an impression upon the sense of smell. If we test them by the principles just laid down, more especially the second, we shall find that, wherever the statements advanced hold good, the substances concerned either are not possessed of sapid properties at all, but lose merely their power of exciting odorous impressions; or by the loss of the odorous impression, where it is strong, or the sapid weak, the latter in its single state falls so far short of the compound sensation previously experienced, that it is disregarded. It is true that we are in the habit of considering such, in many cases, as impressions upon taste, and that the nice distinctions of physiology may fail to overcome the prejudices of prescription; but however this may be, science demands, that in principle at least, if not in practice, the distinction shall be made. In the next place, where the substances employed are really possessed of sapid properties, and do not depend for their sensation upon odorous impressions; it will be found that no interference with the function of smell will prevent their proper sapid impression, and the experiment is so easily made, that it must appear strange that the error should have obtained such currency; a circumstance however, which is easily explained by the confu-

sion which has prevailed between odorous and purely sapid sensations. The point may be easily determined by the experiment of holding the nose, and applying to the tongue solutions of substances which are really and simply sapid, *e. gr.* solution of sugar; of coloquintida; or quassia; when it will be found that such are distinctly tasted; and the effect may be set in still stronger light, by the experiment of inhaling the powder of some of the same substances through the nostril; in which case, if they be not odorous, no impression, save that of contact, will be produced there; but presently after, the taste of the substance will be experienced in its full intensity in the fauces.

It is manifest then that the sensation of proper taste is as independent of smell as from previous reasoning it has been shewn to be distinct from touch. The special nature of the sensation having been established, its conformity to the second test is almost necessary and a matter of course; inasmuch as in every other instance of such a sensation, a special organ is provided for the reception of the impression, and to be the seat of the sense. With regard to this question as affecting taste, it is to be first remarked, that the presence of the sense is restricted to particular parts of the surfaces by which it is exercised, and since the parts enjoying and those devoid of it, derive nerves from common sources, we must look to something peculiar in the organization itself of the former, for the source of the faculty, rather than to a peculiarity in their nerves. Now that a peculiar organ is provided for the exercise of the sense in the tongue, is a generally adopted opinion, it being held that the papillæ fungiformes are the seat of the function. This conclusion appears to have been adopted as well from the exclusive existence of those bodies in the tongue, as from the prevalence of them in greatest number in those parts of the organ most remarkable for the sense, and from the observed erection of them upon the application of sapid agents: nor does there appear reason to question it; only it is to be observed, that the presence of the organ in the peculiar form alluded to is not

necessary for that of the sense, since it is not restricted to the tongue, but is enjoyed by other parts in which the papillæ have not been observed—the soft palate. The organization of the latter part, however, as an organ of taste, has not yet received sufficient attention, and must form the subject of future inquiry.

In conclusion, I feel justified in drawing the following inferences from the experiments and reasonings which have been detailed; 1st, that taste is a special sensation; 2nd, that it enjoys two media of perception; 3rd, that its media of perception are the glosso-pharyngeal nerves, and the lingual and palatine branches of the fifth nerves; 4th, that the glosso-pharyngeal nerves are not its special media; 5th, that the latter nerves both are sentient, and influence muscular action; and 6th, that the spheno-palatine ganglion and chorda tympani have no influence upon either the existence or perception of the sense.

ART. XVIII.—*New Methods of Cure in Cases of Unnatural Openings in the anterior Portion of the Male Urethra.* By J. F. DIEFFENBACH, Extraordinary Professor of Surgery in the University of Berlin.—Translated for the Dublin Journal of Medical Science, by JOHN SWIFT, Esq.

GENERAL OBSERVATIONS.

FISTULÆ, and loss of substance in the male urethra (particularly in the free portion of the member) and vesico-vaginal fistulæ in women, have engaged my closest attention for a series of years. My observations on the latter forms of disease shall be communicated at a future period; at present, I shall only submit the results of numerous and laborious experiments on perforations of the anterior portion of the male urethra.

The frequent failure of my curative experiments might have easily deterred me from further efforts, particularly, as in various countries I had heard from the lips of the most distinguished

teachers of surgery, that openings in the anterior portion of the urethra were almost always incurable. The closure of an opening in the urethra by Sir A. Cooper has obtained universal celebrity; and, indeed, the number of similar successful observations must be very small. I speak here only of perforations of the anterior part of the urethra, from the glans to the scrotum; in the remaining portion of the canal, from the anterior boundary of the scrotum to the bladder, the cure of small and even of large openings, notwithstanding the numerous difficulties connected with them, is comparatively easy. Fistulæ, and even larger openings in the posterior part of the urethra, whether the result of operations, or produced by strictures, infiltration of urine, lodgment of calculi, or unskilful catheterism, &c. are, generally speaking, capable of being cured by merely keeping a catheter in the bladder. I have treated successfully many patients of this description by this plan and the simultaneous use of cataplasms or fomentations. The vast experience and profound knowledge of Rust have thrown much light on this subject. In a case of extensive infiltration of urine, in which the whole scrotum was destroyed by gangrenous inflammation, and the urine escaped from an urethral opening half an inch in length, I have seen, to my great astonishment, the whole mischief repaired by the formation of healthy granulations over the catheter, followed by the re-production of the entire scrotum. But the circumstance which operates most favourably in the cure of deficiencies in this portion of the urethra, is the thickness of the soft parts investing it; beneath, an abundant mass of cellular tissue, and on the surface, the cutaneous layer of the scrotum, easily destroyed, it is true, but also reproduced with great facility.

How differently circumstanced is the urethra in the free portion of the penis! Here it receives an extremely meagre covering from its corpora cavernosa and its thin cutis; the latter not thicker than the skin of the upper eye-lid, and consequently about the thickness of ordinary writing paper, is quite inferior to the cutaneous layer of the scrotum in reproductive

power, and on this account is seldom able to produce a sufficient quantity of granulations for the repair of even moderate urethral deficiencies. Associated with this, is another obstacle of not less difficulty, namely, the constant flow of urine. The incision in the posterior portion of the urethra and neck of the bladder, made in the operation for stone, generally heals with facility; the thick mass of soft parts throws up an abundant crop of luxuriant granulations, on which the influence of the urine which trickles off is too insignificant to produce any remarkable disturbance of the curative process. In the free portion of the penis, the closure of an opening is prevented by the escape of urine, even where the fistula is small; here it deranges every plastic process, whether in case of recent wounds of the urethra, or where inflammation has been artificially produced in the edges of an opening of long standing. The edges lose their natural character, the recently united wound scarcely exudes any more lymph, as soon as a single drop of urine touches it; its edges are again separated, the granulations become extremely scanty, and the process of cicatrization never extends beyond the formation of a slight border of epidermis.

It is necessary to observe, on the other hand, that I have healed recent wounds of the urethra with as much facility as other simple wounds; that I have frequently made incisions into the urethra for the purpose of removing large impacted renal calculi, which did not admit of being crushed, or foreign bodies which had been forced into the canal; that I have also made incisions through the integuments into the urethra, where the penis alone, without the scrotum, was swelled to an enormous extent from infiltration of urine consequent on stricture, and where every other outlet was impossible; or where in cases of stricture a false passage had been made by an unskilful hand; and that in all these instances a cure was frequently accomplished by simple treatment without any unfavourable accident. These exceptions, however, do not prejudice the existence of the general rule.

Cicatrized openings in the urethra, whether they be small

or large, old or recent, are always to be looked upon as phenomena of the rarest kind, and as I have already stated, cures of this description are regarded by many surgeons as quite impossible. He who would assert that a trifling aperture, a hole in the urethra, could be closed by touching it with caustic, or by means of the bloody suture, speaks as a blind man would of colours, or has been favoured by mere chance in a solitary instance. With the exception of a few recent observations on the curative treatment of fissures, or perforations of the urethra, which stand as beautiful living monuments of the surgical art, we possess no information on this subject; at least I am not acquainted with any detailed observations on this point, or any special modes of operation adapted to the various cases to be met with in practice.

I shall now beg leave to communicate the result of my observation on the subject in question. In the first place, with respect to the extent of the opening, I have seen fistulæ so small as to admit only a horse hair or a fine hog's bristle. In other cases, the opening was large enough to receive the end of a thick sound; sometimes the aperture was the size of a lentil or a large pea; in bad cases it measured three, four, or even more lines; or a whole piece of the urethra, with the corresponding integument, was deficient, the loss of substance in the latter being much more considerable than in the urethra, so that there was no cutaneous margin enclosing the defect as in other cases. The most unfavourable case for cure is that in which the evil is the result of congenital malformation, as for instance, partial or complete hypospadias and epispadias. The relative size of the opening, however, affords no criterion as to the curability of the case; this depends much more upon the part of the member in which the perforation is situated. The parts in the neighbourhood of the scrotum are the most favourable; for the skin is extremely abundant and of a firmer structure. Further on, towards the centre of the penis, the difficulty increases. In the vicinity of the prepuce it is still greater; but the cure becomes incredibly difficult and almost impossible, when an opening of

considerable extent is situated close behind the glans, at the place where the frenulum præputii is attached; it is unnecessary to state, that such cases are always attended with destruction of the frenum; the closure of a moderate perforation of the glands itself is much easier. On the other hand, the cure of even a slight degree of hypospadias, where the fissure affects only the glands, is a matter of difficulty. Here, as in cases of epispadias, the difficulty increases with the length of the fissure, and attains the highest degree when it extends into the bladder.

SYMPTOMS CONNECTED WITH PERFORATION OF THE URETHRA.

The inconveniences attendant on any of the former conditions vary according to the extent and situation of the disease. In cases of very narrow fistulæ, where it is often difficult to discover the external opening, a small drop of urine exudes while the patient is making water, and betrays the situation of the aperture; where the opening is larger, of course a greater quantity trickles out, and where its extent is still more considerable, the urine spouts forth in a stream, unless the external opening is overlapped by a fold of skin; where the loss of substance is extensive, the whole, or the greater part of the urine passes off by the false channel. In cases of epispadias, a considerable proportion of the urine runs along the fissured urethra as in a gutter, but the principal part of it runs over. The nearer the fistula is to the anus, and the more extensive it is, the more annoyance does the patient suffer from the wetting of his shirt and trowsers, during or after the act of passing water. In coition, the seminal fluid passes sometimes the right, sometimes the wrong way; it does not, however, appear to make its way through narrow fistulæ. This defect has generally a most unfavourable influence on the patient's mind; I have seen many persons in whom the complaint had existed for years, and who, with the exception of a slight inconvenience in passing water, were perfectly well, who cursed their existence, declared that all their happiness was annihilated, and talked of shooting themselves. None, however, went so far as this to my knowledge.

EXAMINATION OF FISTULÆ AND LARGE OPENINGS IN THE URETHRA.

I have not seen any narrow fistulæ which perforated the urethra in a straight direction from within outwards, as is the case when the opening is large; the canal always ran in an oblique direction from behind forwards and from within outwards. Its greatest length in one case amounted to a full inch; generally speaking, it is about a half or a quarter of an inch long. The internal opening is always much larger than the external. The canal is sometimes of a funnel-shape, particularly where the external opening is so narrow that the skin only exhibits a slight moisture when the patient passes water. Occasionally, not a drop of urine escapes; should this have occurred on the first examination, the patient should be directed (if the minute aperture cannot be discovered) to compress the penis at the glans, and then strain so as to force the urine against the compressed part; a small quantity will then generally ooze out. Having failed on one occasion in detecting the opening by this manœuvre, my patient told me he would shew me a still surer way. He immediately drew out a piece of fine cambric, about half the size of a square inch, laid it over the spot where the opening was most likely to be situated, closed the orifice of the urethra by compressing the glans with two fingers of one hand, while with two fingers of the other hand he pressed the piece of cambric with a moderate degree of force against the suspected spot, and then having forced the urine into the urethra, discharged it suddenly by removing his fingers from the penis. He then desired me to look at the little piece of cambric which was sticking to the skin; it exhibited a small speck of moisture about the size of a pin's head. Guided by this we instantly discovered the opening of the fistula.

The walls of a narrow fistula are, generally speaking, so delicate, that no local hardness or thickening can be felt; sometimes a slight hardness or a small string running obliquely backwards is perceptible to the touch. Not unfrequently, a periodic retention of urine in the fistula takes place, and a

small urinary abscess is formed. If the external and (if I may use the term) imperceptible opening has been previously detected, the elastic integument of the member will allow the point of a fine piece of catgut, carefully scraped and oiled, to be introduced, and when this has been forced in to the depth of some lines, the end is to be cut off at the distance of a few lines from the skin, a small piece of adhesive plaster is to be laid over it, the latter is then covered with a piece of linen roller, and the penis sponged with warm water until the catgut swells up under the bandage. A close examination of the canal can now be made with facility, and a fine probe can be readily introduced. If a full sized oiled sound be previously introduced, with the groove directed downwards, the head of the probe will strike against the groove and cause a slight noise. The breadth and shape of the passage may be known by the aid of a short piece of catheter to the anterior end of which a small roll of modelling wax is attached, as recommended by Ducamp. When the piece of wax has reached the internal opening of the fistulæ, the penis is to be compressed laterally so as to press the wax into the canal. When the impression has been taken, the roll of wax is to be withdrawn and dipped in cold water. The impression of the internal opening only is perfect, and it is the shape of this alone that is requisite. The examination of wider fistulæ is much easier, as in such cases we can introduce the head of a probe with facility and without any further preparation. Where there is a considerable deficiency of the external covering and of the canal itself, the back of the urethra is visible and looks like a piece of red velvet. In such cases a silver female catheter can be passed from the glans through the aperture, and *vice versâ*. In cases where the patient has passed water through the opening alone for several years, the mucous membrane beyond this part gradually ceases to secrete, and the anterior termination of the canal becomes dry. In passing the catheter I have frequently brought away a soft slimy or even cheesy mass; at the same time this

portion of the canal was dried up, narrowed throughout its whole extent, the mucous membrane indurated, and great insensibility present. In other cases where the disease was more recent this portion of the canal was extremely sensible.

VARIOUS MODES OF CURING FISTULÆ AND OPENINGS IN THE ANTERIOR PART OF THE URETHRA.

The closure of openings in the urethra is effected either by the production of granulations on the edges, or by an immediate union; inflammation is excited around the opening by caustic applications, or the edges are pared and then united by sutures.

Experiments on narrow Fistulæ.—The closure of a narrow fistula of the urethra is just as difficult as it may appear at first sight easy. I have often employed cauterization and the bloody suture in the same individual without success. The experiment with caustic is made in the following manner. The narrow canal of the fistula having been dilated to the size of a raven's quill, by passing a thin piece of catgut through the external opening into the urethra, and if the opening be close to the glans, drawing out the piece of gut through the latter, a pencil dipped in concentrated tincture of cantharides is to be introduced into the fistulæ; the pencil is then to be twirled round and kept in for about half a minute, so as to touch every point sufficiently. Before this application is made, a wax bougie should be passed into the urethra, partly to prevent the tincture from getting into the urethra, partly to enable the surgeon to manage the penis more conveniently. The bougie should be of moderate thickness; if it be too small, the tincture will easily pass between it and the urethra, and if it be disproportionably large, the fistulous passage will be compressed by it, and the pencil cannot be passed to a sufficient depth. The caustic application is to be made three times within the space of six or eight hours, (merely moistening the pencil each time,) the wax bougie is then to be withdrawn, and a middle sized elastic catheter introduced, which is to be fastened to the penis

with pieces of narrow tape. It is unnecessary to mention, that the patient must lie quietly and lightly covered on a bed or sofa. Next day, the loosened epidermis is to be removed with a small forceps, and a small piece of dry sponge is to be passed down to the bottom of the fistula, and twisted round several times so as to irritate the surface of the wound and brush off the rest of the epidermis. A few days afterwards, when suppuration has commenced, the application of the tincture of cantharides is to be repeated, until a sufficient crop of granulations is produced. The internal treatment is to be moderately antiphlogistic.

I have seen two patients in whom a large fistula was converted into a smaller one by touching the external opening with lapis infernalis. At first the external opening closed, but the urine forced its way through the larger internal aperture, and the external one opened again. I have rarely, however, succeeded in effecting a cure by the foregoing method, in spite of all the care I could employ. Even where the external opening was closed and completely covered with epidermis, it burst open again, when the patient neglected to pass water for a long time, and a strong gush of urine came suddenly from the urethra, or during the act of coition. The patients lost all hope or inclination, particularly after a second trial, and I saw no more of them.

Remarks on the Simple Suture after Cauterization.—Having produced inflammation in the canal by the application of tincture of cantharides, I introduced a fine curved needle through the integuments, at the distance of from a half to a full line from the edge of the opening, passed it deep into the opening, and having tied the ends of the thread together, cut them off at some distance from the knot. I took care to pass the needle obliquely through the depth of the fistula, in order that the point should touch the catheter at the internal opening, so as to unite its edges also. This mode of operating, which I tried three times, failed in every instance. Violent inflammation came on, and one patient had a slight infiltration of urine

into the cellular substance; this, however, was removed by a slight incision and a poultice. In every case the ligatures cut through the soft parts, and had to be removed on the third day. The fistulæ were now of considerable size, and I entertained the hope of being able to cure them by the suppurative process, but this also failed. In the third patient, a shoemaker, aged twenty-two, I affected a complete closure of the fistula by touching it repeatedly with caustic. The patient was three months under treatment, during which he did not use the catheter, and followed his ordinary business.

Remarks on the twisted Suture after Cauterization.—The caustic application was used as in the foregoing cases; the fistula was dilated with a piece of catgut, and tincture of cantharides applied with a camel's hair pencil. In the case of a young student of theology, aged 22, a fine needle was introduced about a line from the edge of the opening, passed across the fistula, and the ligature twisted round it; the ends of both were then cut off. Every thing seemed to go on well. On the third day the needle was very loose, but I did not disturb it. On the fourth it came away together with the ligature, and the opening now appeared considerably enlarged. By dressing it with a stimulating ointment it subsequently diminished to its former size, but the young man would not submit to any further experiments. Since that period I have never operated in this way in any case.

Formation of a raw Surface with the Knife, and Application of the simple interrupted Suture.—Having dilated the fistula, which was situated in the anterior part of the penis of a merchant, aged 28, I introduced a small knife, the blade of which was about the breadth of a straw, until the point rested against a wax bougie previously passed into the urethra. I then made the octagonal handle of the knife rotate several times on its axis, so as to make the blade act like a wimble on the walls of the canal. A few drops of blood came away. The loosened epi-

dermis was then drawn out with a small forceps, and the attaching fibres divided with an eye scissors. A suture was now applied, and cold applications made; but suppuration took place, and the ligature cut its way through. The patient, after having been brought back to his former state, said he was much obliged to me, but did not wish any further treatment. I had cauterized him a few years previously, according to my own method, without success.

I have operated on many other patients under exactly similar circumstances, and in the very same way. In one patient only, a man aged 40, the cure succeeded; not, however, by union by the first intention, but by suppuration. The ligature cut its way through on the third day, and a slight infiltration of urine took place, which was removed by a small incision. The catheter was used for four weeks, at the end of which the fistula was permanently cured.

Formation of a raw Surface and Application of the twisted Suture.—In the case of a young and amiable nobleman, who suffered a great deal of anxiety about a very small fistula in the anterior part of the penis, I introduced a fine needle through the integuments (having previously stripped the edges of the fistula of epidermis) about a line from the margin of the opening, and passed it deep through the cellular tissue and across the canal. I then twisted the thread round the needle and cut off its ends, as well as that of the needle, close to the skin. I did not introduce the catheter. After a few days the ligature became loose and was removed; no urine came away, but on the day on which I thought the fistula was cured, a drop gushed out, and the patient objected to any further operation. I was more successful in the case of a man, aged 30, who had a fistula close to the prepuce. I converted the external opening into an incised wound two lines in length, and then passed the needle through the deep seated parts, without piercing the external integument. The ends of the ligature were then gently twisted

round the ends of the needle, which rested chiefly against the edges of the skin, and held them apart so as to leave a gaping wound externally. With the head of a large silver probe, passed into the urethra, I could feel the needle, by the central portion of which, it is probable, the internal opening was accurately united. I did not introduce the catheter, and limited the treatment to cold applications. To my great satisfaction, the cure was quite successful. On the fifth day, I carefully removed the needle, over which and the ligature a mass of luxuriant granulations had projected in such a manner as to conceal them. I found it necessary to cut away the thread with a fine scissors before I could remove the needle.

MODES OF OPERATING IN CASES OF ACTUAL LOSS OF SUBSTANCE IN THE ANTERIOR PART OF THE URETHRA.

The difficulties attendant on the cure of large deficiencies in the urethra, are much greater than in cases of small openings. In old cicatrized perforations of the canal, we invariably fail in atttempting to produce luxuriant granulations and closure of the opening by means of caustic applications. The common bloody suture I have also found to be so uncertain a means of cure, that I have been obliged to make trial of other methods.

Application of the simple interrupted Suture.—I made the first trial of the interrupted suture on a book-binder, aged 36, who had an opening in the middle of the penis about the size of a pea, through which the urine passed; part of it went through the glans. Having introduced a catheter, I detached the edges of the skin round the opening, and removed a small strip of each, so that the ends of the wound terminated in a sharp angle. I then applied four sutures, and ordered cold applications, an antiphlogistic diet, and perfect rest. Violent inflammation of the skin took place, and on the third day a few drops of an urinous milky fluid were squeezed out. On the fourth day the ligatures became loose, and after they had been removed, the ash-coloured edges of the wound flew open, and became

cicatrized in the space of a few weeks, leaving the patient exactly as he was before the operation. Three months afterwards I persuaded him to make a second trial. It was just as unsuccessful as the first.

Operation for removing a Defect in the Urethra by covering it with two Folds of Skin.—Employment of the twisted Suture and lateral Incisions.—A gentleman, aged thirty-one, living in the neighbourhood of Berlin, had frequently laboured under gonorrhœa and chancre. Many years previously, he had had infiltration of urine, followed by an abscess in the penis, which burst and terminated in destruction of a portion of the urethra. From this time the patient was subject to every species of suffering. On making an examination, I found, that although the deficiency in the urethra was full half an inch in length, the flow of urine was greatly impeded by a stricture. The consequences of the stricture were, the formation of disease in the posterior part of the urethra, perforation of the same at several points, infiltration to a considerable extent into the cellular tissue of the scrotum and perineum, which appeared hard and callous. He had also several fistulous openings, three of which were situated in the vicinity of the anus, and one in the perineum. From all of these a thin, milky, offensive urine oozed out in drops. By means of bougies and catheters, gradually increased in size, (at first the stricture would admit only the very smallest,) and by large warm cataplasms, I succeeded in removing the desperate condition of the patient. The dilatation of the stricture was followed by a free discharge of urine, the infiltration into the perinenm and scrotum ceased, and the fistulous openings healed. I then proceeded to dilate gradually the anterior portion of the urethra through which no urine had passed for many years. This portion of the canal had dried up and become impassible to such a degree, that a fine bougie could not be introduced from the glans ; from the false opening in the urethra it could be more readily passed

forwards. The irritation, however, produced by the introduction of the small bougie, was so great, that the prepuce became excessively swollen, and formed a paraphimosis. This symptom was removed by the use of cataplasms, and liquor plumbi subacet. dilut.

As soon as this portion of the canal had been restored to its proper calibre, I considered that the period had arrived for making an attempt to close the opening, especially as the patient was now free from any difficulty whatever in passing water. I began, therefore, by introducing an elastic catheter from the glans. As my object was to obtain a surface of epidermis for the deficiency of the urethra and also for the outside, I raised a fold of skin on the right and left side of the opening, and to make the covering more secure, I produced the folds anteriorly and posteriorly half an inch beyond the aperture. I now passed the needles* through the folds and pressed the edges of the skin together. By this manœuvre I was enabled to pare the edges of the wound quite evenly with a sharp scissors. The ligatures were then wound round the needles, and the edges of the wound brought into the most accurate contact; lastly, each end of the wound, which terminated in a sharp point, was secured with a fine needle. The operation was finished by making two longitudinal incisions, one on each side at some distance from the sutures, and as these extended beyond the sutures, all tension of the skin immediately ceased.

Notwithstanding a strict internal and external antiphlogistic treatment, violent general reaction took place, commencing with a severe rigor. This state was somewhat improved by a bleeding from the arm. The penis and scrotum were greatly inflamed. A second bleeding on the third day after the operation was followed by more favourable results. The edges of the wound appeared united, and no urine seemed to have

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* Very small fine needles (Insektennadeln) were used in these operations.

escaped between the catheter and the urethra. I removed a few needles at the ends, and the edges of the wound held together. On the fifth day, I found the rest of the needles partly loose, and took away some of them. At this situation the edges of the wound immediately separated for the greatest part of their length. Here and there the connexion was still maintained by a few bridges. The cellular tissue, at the separated points, had a dark colour; a slight degree of pressure on the penis forced out a quantity of infiltrated urine. The swelling of the scrotum had been produced by the same cause. On the following day, the edges of the skin had separated still farther, leaving two openings, each about a line in length, through which the interior of the urethra could be seen. On removing the two last needles, between which the edges of the skin had not united, the connected portions gave way, and the edges separated throughout their entire length. Eight days after this unfortunate occurrence the lateral incisions suppurated, and the bridges of skin formed two prominent swellings at the sides of the opening.

This patient was one of the most ill-bred men I ever met with in the course of my life. I would have certainly abandoned him to his fate on the first day of treatment, had I not irritated the opening in the urethra, and hoped that I might be able to make a contribution to science. It is enough to say, that he was one of those low natures who would disgust us with our profession, were we to meet with such very often.

This, my first more important operation for remedying defects of the urethea, has been circumstantially communicated in my "Surgical Experiments on the Reparation of Mutilated Parts of the Human Body, according to New Methods; Berlin, Eslin, 1829. Part 1." I have repeated it briefly here, in order to render the subject more complete.

Union of the simple Edges of the Skin over the Deficiency in the Urethra, and lateral Incisions.—In a second case,

which differed from the foregoing in being more manageable, I tried the same operation, but without effecting a perfect cure. A merchant, aged 40, had an oblong opening in the middle of the penis, about the size of a large pea; it formed a kind of transverse slit in the urethra, and the edges were somewhat indurated. The patient stated that four years previously he had fallen against the sharp edge of a commode, and received a severe contusion, the consequence of which was gangrene, and the formation of the opening already described. I converted the transverse shape of the opening into an oblong one, detached the skin on both sides, and united the edges by means of four fine needles, round the ends of which the ligatures were twisted, so as to apply the wounded surfaces of the skin to the urethra: I then made the incision on each side. On the third day every thing had a favourable appearance. In the evening the penis became greatly swelled; several leeches were applied over the pubic region, and cold lotions were constantly applied. Next morning matters were in the same state, and I now perceived, too plainly, that a small quantity of urine, which had escaped by the sides of the catheter, had forced its way into the cellular tissue and produced these symptoms. I therefore removed the loosest needle. The edges were united by gray gangrenous cellular tissue, and the rest of the wound was in the same state, showing that the urine had destroyed the newly formed connexion. Warm chamomile fomentations and liquor plumbi subacetatis dilutus soon removed the inflammation; and a few weeks afterwards the patient's state was exactly the same as before the operation.

I cannot forbear to mention here a foreign observation, which deserves to be cited on the present occasion. During my daily visits at the Hôpital du Midi, in Paris, M. Ricord showed me a man of middle age, who had a considerable deficiency of the urethra, about the middle of the penis, of full half an inch in length. The edges were completely cicatrized and dilatable; at the bottom, the red, moist, posterior wall of

the urethra could be seen. The whole of the urine was discharged through this opening. The anterior portion of the canal was nearly in the natural condition, and tolerably moist. I was very anxious to operate in this case, but M. Ricord said he wished to operate on the man himself, and begged that I would communicate to him my views of the case, and assist him in the operation. The case was more interesting on this account, as M. Ricord told me the patient had been operated on twice before without success, by a distinguished Parisian surgeon, according to my method of suture with lateral incisions; and, in fact, two goodly cicatrices were visible on each side of the penis, extending nearly its whole length. (A double compliment, and yet no compliment to me.) The operation was commenced in the presence of a crowd of French, English, and German surgeons and students. M. Ricord first introduced an elastic catheter into the bladder, and then dissected off the cicatrized edges of the opening with great dexterity. The wound formed an ellipse terminating in two sharp points. The cellular tissue, under and in the vicinity of the edges, was separated by horizontal strokes of the knife, and the union was finally accomplished by means of a row of fine needles. M. Ricord conducted every step of the operation in the most perfect accordance with my views; and I can assert that a more masterly twisted suture could not be seen. The ends of the needles and ligatures were cut short, cold applications made, and an antiphlogistic regimen prescribed. The result was, unfortunately, what I have so often witnessed. During the three first days the operation promised a brilliant termination; the skin was moderately red, in the little intervals between the needles the edges lay in beautiful apposition, and had been united by coagulable solidified lymph. The sutures then became surrounded by a dark red border, a slight degree of pressure caused some drops of milky fluid, having an urinous smell, to ooze out; some of the needles became loose, and when they had been removed, the edges separated, exhibiting a

gray surface, and a covering of gangrenous cellular membrane; at the bottom, through the open intervals, the black elastic catheter could be seen. On the following days the rest of the sutures were removed: the edges still remained partially united by a few bridges; these were now the only remaining hope. To support them, small strips of adhesive plaster were applied round the penis, but the urine and the frequent erections destroyed these delicate connexions also, and ere many days had elapsed, the edges of the wound became cicatrized as before.

By a change in the mode of proceeding last related, (which I shall detail after the introduction of the following remarks,) I think I have come nearer to the solution of the problem. In almost all cases, the urine coming into contact with the internal surface of the wound, was evidently the cause which destroyed the healing process, even where during the first days an actual union had taken place between the tender edges of the integuments of the penis. I have remarked that a few drops of that fluid, escaping by the catheter, are sufficient to render the whole operation ineffectual. It may be asked why the cure, which proceeds favourably at first, should, after a few days, be disturbed by an occurrence of this description? In reply to this, I may observe, that during the first days the catheter is grasped firmly by the sphincter muscles of the bladder, so that none escapes; but after this period the catheter excites a considerable degree of irritation in the urethra and bladder, and at the same time is embraced less firmly, so that a small quantity of urine, probably only a few drops, escapes between it and the urethra. In cases of this kind, letting the water constantly flow into a glass, or frequently opening the tube, makes no difference. It might be supposed that this evil could be remedied by changing the catheter, or rather by emptying the bladder frequently during the day; this, however, is followed by still more unfavourable consequences. The tension and relaxation of the penis, the introduction of an instrument into the irritated urethra, and the passing of it over the internal raw and recently

united surface of the urethra, are circumstances alone sufficient to frustrate the whole result. Again, when the catheter is withdrawn, a few drops of urine always follow, and a still larger quantity escapes from the opening in the instrument, even when the mouth is kept accurately closed with the thumb.

From the failure of an experiment for the cure of a considerable defect of the urethra, by covering the opening with two folds of skin and making lateral incisions, (an account of which has been published in my surgical experiments, and also briefly noticed in this paper,) this secret infiltration of urine, recognizable only by its effects, led me to think of some other channel for the urine besides the whole length of the urethra, so that a single drop should not touch the united parts. In the first part of my surgical experiments, I threw out the hint that the operation might possibly succeed if an opening were made nearer the bladder at some distance from the defect in the urethra, and a catheter introduced through this opening into the bladder before the operation was undertaken. This appeared to me at the time very plausible, and I have no doubt, even at present, that the healing of the larger opening could be successfully accomplished. The results, however, of some cases in which I was not able to succeed in closing small urethral fistulæ, prevented me from putting this method of operating into practice. It appeared to me on this account very probable that the vent-opening would be converted into a fistula, although the defect in the urethra might be cured, and this appeared so much the more probable, as it would be necessary to give up the use of the catheter for a considerable time. It is curious, that during my stay in Paris, I learned from several young French surgeons that one of their countrymen had, a short time previously, entertained the same idea which I had published seven years before. Whether that practitioner has performed the operation, and with what results, I know not; but I have sought for it in vain in the more recent French journals.

*Employment of the Twisted Suture in Urethral Defects. Lateral Incisions, and complete Detachment of the Bridges.**— The idea of the foregoing operation, which I had previously turned in my mind, namely, by employing a representative fistulous opening, to carry off the urine from the inner raw surface of the edges of the skin covering the opening, as I considered that the urine by touching them was the principal cause of the failure of most operations; this idea I realized in a less hazardous way, viz. by detaching the bridges formed by the lateral incisions. I am extremely happy in thinking, that by this method I have solved the problem of closing moderately large openings in the urethra. The operation is performed in the following way:—

1. An elastic catheter having been introduced into the bladder, the edges of the opening are seized with a hook forceps and removed with the scalpel, so as to form a wound terminating anteriorly and posteriorly in a sharp point. The surgeon then lays hold of the edges again, and detaches the skin from the subjacent parts, to the extent of three or four lines around.

2. As many twisted sutures are then applied as may be necessary to close the fissure.

3. The lateral incisions on both sides of the penis, parallel to, and double the length of the suture.

The next step is the separation of the bridges from the parts beneath by means of horizontal strokes of the knife, carried under the skin from the lateral incisions, during the performance of which operation the edges of the skin are held with a hook forceps, and put on the stretch, so that by lifting up the bridges the catheter in the urethra can be seen.

I performed this operation after the manner detailed in a case of urethral defect. L. F., a shopman, greatly reduced by all kinds of debauchery, who had frequently laboured under

* This term is applied to the lateral flaps of skin, the inferior surface of which was detached from the subjacent parts.

gonorrhœa and chancre, and finally under general lues, of which he was cured by mercurial frictions and the use of Zittmann's decoction, had an urethral opening about the middle of the penis, capable of admitting the largest sized catheter. According to his statement, an ulcer had eaten its way through the urethra from without; and from the state of the parts around the opening this appeared to me extremely probable. Experiments had been previously made by other practitioners, to effect a closure of the opening by caustic, but had proved unsuccessful. The operation was performed exactly in the way I have just described. During the first days, nothing important occurred, except tumefaction of the integuments of the penis, and particularly of the bridges. Notwithstanding frequent nocturnal erections, the suture did not experience the slightest disturbance, for the detachment of the bridges obviated all tension. On the fourth day, I removed a needle from each angle of the wound, and found that here the union had succeeded. The under surface of the tumefied bridges was covered with granulations, and towards the ends I perceived that these surfaces, in the direction of the mesial line of the penis, adhered to the subjacent parts; in fact, so firmly, that when I had removed all the needles, the bridges did not return to their old places. I should not omit to mention, that some urine had escaped beside the catheter, and had passed off through the lateral openings. On the fifth day I removed the four needles, and found the edges had united with the exception of an opening about the size of a mustard seed; five days afterwards, this closed by touching it with tincture of cantharides. The lateral incisions were gradually filled up by granulations. On the fifteenth day there was no longer any communication with the urethra, and in three weeks the cicatrization was complete.

Great as my joy was at the success of this operation, I was still more delighted in arriving at the solution of the enigma, where the question turns particularly on operations necessarily

different, and this is as follows. The thin cutaneous edges of every urethral opening afford such insignificant points of union, and their tendency to unite by the first intention is so weak, that little dependance can be placed on them; on the other hand, the tendency of the parts in question *to adhere with their under surface to* another raw surface is, from the luxuriance of the cellular tissue, uncommonly strong; in other words, *the tendency to an union of edges is slight, to an adhesion of surfaces very powerful.* The plan, therefore, which I laid down for myself in all difficult cases for the future (holding in view the difference of circumstances) was this, to form a large raw surface and to cover this with the detached skin, and then to unite the edges with sutures, laying, however, less stress upon the latter part of the operation.

The Splint Suture.—In cases of larger deficiencies, particularly where there is a smaller edge of skin on both sides, a complete closure of the opening would not perhaps be effected by the foregoing method; here the following change in the operation might be of assistance.

1st. Paring the edges of the skin so that they can be united, and separating the subjacent cellular tissue.

2nd. Union of the edges by the continued suture.

3rd. Lateral incisions and complete detachment of the bridges, which, according to this method, are one-third broader, so that the catheter in the urethra may be seen.

4th. The application of two leather splints along the bridges, of the same length as the latter, in order to press the two bridges with their raw surfaces together and retain them in this position. The two little splints should be three lines in breadth and of moderately stiff leather, and have each three eyelet holes. The surgeon applies them to the inner edge of the bridges on both sides, and presses them together, along with the skin, with his left finger and thumb; then passes the needles through the holes in the splints, and at the same time

through both bridges, and finally rolls the threads round the sharp ends of the needles, having previously shortened them a little, as I am accustomed to do in the Taliacotian operation. The parts must be drawn together with but a moderate force, in order to prevent mortification of the bridges in case of swelling. By this mode of operating the result in intricate cases will, I think, be more certain. If, in case of a very small penis, the operator should, instead of the lateral incisions, make one long incision on the dorsum of the penis, and then completely detach the skin on both sides, the result would certainly be unfavourable, as the urine which might escape by the catheter could not flow off. The splint suture has a very great similarity to the old tennon suture. I have not had as yet any opportunity of trying the practical value of this method. I hope it will succeed.

Method of operating by Transplantation in Cases of Urethral Deficiencies.—By the last mentioned modes of operation I have been brought still nearer to a transplantation of the skin to the defective parts. Still that was only removing and drawing away the skin in a straight direction from one place to another in its immediate vicinity. By the operations to be detailed presently, the skin was drawn from a greater distance, or turned laterally, or twisted round as a flap with a peduncle, and thus transplanted. The various modes of transplanting were determined, first, by the situation of the urethral defect; secondly, by the size of the deficiency. In cases of considerable loss of substance in the posterior portion of the penis, the skin of the scrotum is most conveniently employed; in cases of extensive destruction of the urethra about the middle of the penis, the skin from the dorsum of the penis; in openings in the anterior portion close behind the fold of the prepuce, the skin covering the central portion of the organ; and in perforations close behind the glans under the foreskin, the prepuce itself.

Transplantation of the Skin of the Scrotum over an Opening in front of the Scrotum.—1. An elastic catheter is to be introduced into the bladder. The edges of the opening are then seized with the hook forceps, and dissected off in such a manner, as to form a transverse wound with two sharp angles; the latter extend as far as the middle line of the sides of the penis. This removal of two lateral portions of skin is necessary, in order that the skin of the scrotum may be drawn over the vacant spot without any puckering.

2. The surgeon then pinches up a longitudinal fold of the scrotum behind the opening, in the direction of the raphe, and divides it transversely to the extent of two inches. The isolated band of integument in the middle is to be raised from its subjacent connexions by horizontal strokes of the knife.

3. The transverse bridge of skin is then drawn forwards, and its anterior margin united with the end of the skin of the penis, by five or six twisted sutures. The boundary of the part deprived of skin forms the posterior margin. Under this a piece of elastic bougie, two inches in length, is to be passed as far as the opening in the urethra, to afford exit to any drops of urine that may escape.

This method has certainly the advantage over the transplantation of a pedunculated flap of scrotal integument, which has been hitherto chiefly employed, but which readily shrivels up and dies. The bridge which I form, while it covers the opening, retains its attachments, in consequence of having two firm points. The sutures are to be removed after a few days, when the union of the parts is completed. Should the edges of the skin separate at a few points, they may be supported with strips of adhesive plaster. The vicarious opening heals last. Dr. Magnus has been so kind as to send me a very interesting communication respecting an operation for transplanting a flap of the skin of the scrotum, in a case of extensive urethral defect, undertaken by M. Ricord, and performed by him some months after I had

left Paris. The loss of substance in the urethra amounted to more than an inch. M. Ricord having removed the edges, dissected a large piece of skin from the scrotum on one side of the raphe, twisted the flap round, applied it against the opening, and fastened it with a number of sutures. The operation failed in consequence of the flap being destroyed by mortification, although M. Ricord paid every possible attention to the operation, and the after treatment. Dr. Magnus mentioned also, that M. Ricord had tried this operation once before on the same man without success. This could not, however, be the same individual on whom M. Ricord operated before in my presence according to my former method. I shall inquire more closely into this matter.

Making the whole Skin of the Penis revolve on its Axis for the Purpose of covering larger Deficiencies in the Middle of the Urethra.—This mode of operating is the most difficult and important of all hitherto described. I have designed it for cases of considerable loss of substance in the urethra, situated in the middle of the penis, and particularly where the skin has been destroyed round the opening, and where, consequently, it becomes impossible to draw the edges over the opening, even after the formation of lateral incisions and detachment of the bridges.

1. Detachment of the edges of the skin round the opening from the point where the operator raises them with the hook forceps. The edges are then to be separated from their connexions around as far as can be done with ease, but none of the extreme edge is to be removed.

2. A longitudinal fold of skin is then to be pinched up at one side of the root of the penis and divided transversely. The incision must form two-thirds of a circle which encompasses the penis. The external lamina of the prepuce is then to be drawn back a little, and the skin at the opposite side of the penis, behind the corona glandis, pinched up in like manner into a longitudinal fold, and divided transversely to the extent of two-thirds of the skin encompassing the organ.

3. The greater part of the integument of the penis lying between the two incisions is then to be detached. This is done most expeditiously by raising the edge with a forceps, and dividing the loose cellular tissue beneath with a sharp eye scissors.

4. As soon as the annular portion of skin between the two semicircular incisions (which comprises the chief part of the integument of the penis, and is generally about two inches in breadth) has been detached, it is to be turned round completely so that the sound skin from the dorsum of the penis is brought underneath and covers the opening, while the opening in the skin is transferred to the dorsum of the penis. Should this part of the operation produce any dragging of the skin at the posterior and anterior angles of the wound, an incision may be made through it in a longitudinal direction from the angles of the wound, backwards towards the root of the penis and forwards into the prepuce.

5. Making points of interrupted suture at the anterior and posterior wounds, is chiefly useful for the purpose of preventing the spontaneous return of the skin to its former position. Small strips of very good adhesive plaster, carefully applied, contribute not only to support the sutures, but also, what is of equal, and perhaps greater importance, press the skin gently against the penis, and prevent the formation of a layer of coagulated blood between them. Hence it is necessary to pour a considerable quantity of cold water over the parts beneath, before the edges are brought together, and not to use the needle and thread until no more blood appears. A piece of fine elastic bougie is to be passed under the posterior edge of the wound, at the under side of the penis, as far as the opening in the urethra, to give exit to any urine that may escape beside the catheter. In this method of operation it is indispensably necessary that a skilful assistant should remain constantly by the patient. In case of nocturnal erections, the strips of adhesive plaster must be divided without being removed, lest their tightening should injure the skin.

On the fourth or fifth day, when a proper adhesion of the skin is perceptible, the sutures are to be removed; but the application of strips of adhesive plaster must be sedulously continued. When the cure is nearly accomplished, the bougie, which had been changed twice a day, is then no longer introduced. I shall communicate the result of this mode of operating at a future period in this Journal.

Transplantation of an annular Fold of the Prepuce backwards, to cover an urethral Opening situated behind the Prepuce.—When the urethra is perforated close behind the prepuce, the annular transplantation of the external layer of the prepuce is indicated. The operation is performed in the following manner:—

1. The external layer of the prepuce is pinched up in a large longitudinal fold in front of the opening, and cut across. If the incision through the skin has not embraced two-thirds of the circumference of the penis, the wound is to be extended a little on both sides towards the dorsum of the penis. In the next place, another longitudinal fold is to be pinched up behind the urethral opening, more or less close to it, as the skin allows, and divided obliquely, so that the two ends of both incisions form an oval island, terminating in two sharp points, in the middle broad portion of which the urethral opening lies. The isolated portion of skin is then to be removed by horizontal strokes of the knife.

2. The cut edge of the prepuce is then to be raised with the hook forceps, and the cellular tissue which connects the external with the internal layer, divided with an eye scissors. When this is done, the frenum and the internal layer of the prepuce are to be divided, so as to form an opening about half an inch in breadth; this serves to carry off the secretion of the wound, and allows the introduction of a small piece of bougie to obviate infiltration of urine. The posterior cut edge of the external surface of the prepuce is then to be drawn backwards, so as to

bring it over the urethral defect, and unite with the corresponding edge of the skin of the penis. Seven or eight sutures are necessary for this purpose. Small strips of adhesive plaster, crossing each other from below upwards, serve to make faster the annulus of skin. When the prepuce is very narrow, it will be also necessary to divide both layers of the prepuce as far as the corona glandis. There is nothing peculiar in the after treatment. The urine which escapes by the catheter will find its way through the opening in the internal layer of the prepuce.

Annular Transplantation of the Skin of the Penis to cover Urethral Openings close behind the Glans where the Prepuce is deficient.—The closure of large urethral openings immediately behind the glands, belongs indisputably to the highest propositions in surgery. Hitherto, no man, to my knowledge, has been freed from this unpleasant condition. The natural points of attachment for the skin of the prepuce which is to be transplanted, are here wanting. The edges of such an opening, as well as the surrounding parts, are hard and callous; as it generally happens that an inveterate chancre at the frenum, in cases of narrow prepuce, first destroys the latter, and then eats its way through the urethra. But the case is much more difficult when the patient has no prepuce, (as, for instance, the Jews,) and there is a perforation in the part of the urethra already mentioned, as occurred in a case to be detailed hereafter. In such instances there are no materials in the vicinity for closing the opening; as among the Jews not only the two layers of the prepuce, but also a considerable portion of the integument behind it, are removed in circumcision.

The following operation is the only one which affords any hope of relief under such circumstances:—

1. The callous edges of the opening are carefully dissected off, and a small wedge removed on both sides. If the glans be now depressed, the edges mutually approach, and the opening forms only a fissure. Two fine sutures are immediately applied

in this situation. When the first has been tied, one end of the ligature is to be cut off, and the other passed from the opening out through the urethra, by means of a straight needle rounded off at the point. After tying the second suture and cutting off one end, the other is passed with the needle, in the centre, between the approximated lips of the wound, and brought out in like manner through the exterior opening of the urethra. From such a closure of the urethra, however, as this, it would be very wrong to expect anything.

2. The operator, in the next place, pares not only the immediate circumference of the opening, but also the whole under surface of the glans, to within a line of the urethra, by raising the superficial cutaneous layer with a fine hook forceps, and dissecting it off with a small scalpel. The boundaries of this dissected surface are marked by an incision passing perpendicularly into the substance of the glans, in order that the edge of the skin to be transplanted may be applied against another cut edge, and not lie flat on the glans. This, therefore, forms a flat insertion. The shape of the whole pared surface is that of an oval cut transversely. The rounded end looks towards the opening of the urethra; the straight line is directed backwards; and the angles at each side extend as high as the dorsum of the penis.

3. The flap of skin which is to cover the perforation is taken from the posterior and inferior part of the penis. The skin in this situation is raised into a fold, and divided obliquely. The ends of the wound run in an oblique direction upwards and forwards at the top, in order that the annulus of skin may be more easily drawn towards the anterior part of the penis. The skin is then to be detached from the subjacent parts, leaving it in connexion with the penis upon the dorsum only.

4. *Application of the Sutures.* The anterior edge of the annulus is drawn over the pared surface with the hook forceps, and carefully united with the cut edge of the glans by fine

twisted sutures. From its thinness and pliability it accommodates itself to any form required. The posterior raw surface is covered with fine charpie, and a few strips of adhesive plaster applied to support the dressing. A piece of fine bougie is to be passed under the posterior edge of the annular portion of skin, to carry off the urine that may escape beside the catheter. The ends of the ligature, which hang from the opening of the glans, are to be removed as soon as they have cut their way through.

Annular Transplantation of the Skin of the under Surface of the Penis, to cover an urethral Opening behind the Glans where the Prepuce is deficient.—It is true, I have performed the operation, according to the method detailed here, without success; probably, it would have succeeded, had not several unfortunate occurrences taken place. The patient was a Jewish shopman, aged twenty, in the enjoyment of full health and strength. The perforation in the urethra was produced by a bad syphilitic ulcer which appeared first at the point where the frænum is attached to the glans. Dr. H. Berends had succeeded in curing the patient completely by a proper mercurial course.

I found an opening in the urethra close behind the glans, capable of admitting a large pea; the edges were callous. Through the aperture the superior wall of the urethra could be seen of a deep red colour. The principal part of the urine passed through this perforation. The young man was so anxious to be cured, that he awaited the operation with impatience. I did not by any means conceal from him the difficulties of the operation, as well as the uncertain nature of the result. To avoid repetition, I performed the operation according to the rules given above; the opening in the urethra was united by itself; a large raw surface was formed on the under part of the glans and to some distance behind it, and a broad annulus of skin was drawn from the under surface of the penis

over the glans and united by means of fine needles. Notwithstanding his good intentions, the patient made the operation very difficult to me by his restlessness and struggling; still every thing succeeded so well, that the most accurate union of the parts was effected. The treatment was strictly antiphlogistic, and cold applications were continued with the most sedulous attention. The union was nearly complete, with the exception of a point of suppuration here and there. The cold applications were changed for warm lotions of acetate of lead and the swelling and redness of the skin disappeared. On the fourth day most of the needles were removed, and the flap still maintained its attachment, when an extremely unfortunate occurrence again destroyed every thing. The patient could not guard against erections, which began as soon as he closed his eyes; finally, he had a pollution, during which the catheter was forced out of the bladder in spite of its fastenings, and remained sticking in the urethra in the form of a strongly curved arch. The seminal fluid had escaped partly through the catheter and partly beside it. The fresh connexions could not possibly withstand this involuntary violence, and a separation took place at all points. I entertained a hope of being able to accomplish an union by means of strips of adhesive plaster; but as this did not succeed, I endeavoured to preserve the skin, and with success; it returned partly to its old situation, and the rest of the wound healed by granulation. The young man now found himself exactly in the same state as before.

Closure of a large Opening of the Urethra behind the Glans by Transplantation of the Prepuce.—During my stay at Paris, I had the good fortune to form an acquaintance with Dr. Aronssohn, a distinguished surgeon and physician of Strasburg. This gentleman, who was of great assistance to me in several important operations, proposed on this occasion, that one of his patients, who laboured under a considerable loss of substance in the urethra, should come from Strasburg to Paris, and be

introduced to me, in order to undergo an operation if there was any chance of a cure. Soon afterwards M. S. came to Paris. H was a tall, fine-looking man, and had a short time previously been an officer in the French army. His excitement bordered on madness, and his complaint had brought him almost to despair, for the most distinguished surgeons had openly stated to him the incurability of his disease. On making an examination, I found a considerable perforation of the urethra about the size of a small bean; there was not only an opening in the inferior wall of the canal, but the lateral walls were also deficient, and the urethra looked as if it had been dissected out at this point with a knife. The upper wall which remained had the appearance of red velvet. The parts around formed a hard, callous, flattened cicatrix. As the prepuce was long and thick, it covered the defect completely in the ordinary state, so that no part of it could be seen; but when it was completely drawn back the opening was quite visible. This perforation of the urethra, at a place so unfavourable for a cure, was the consequence of a large, obstinate, syphilitic ulcer. The patient had laboured for a long time under general lues, of which he had been completely cured by Dr. Aronssohn. I did not conceal from the patient by any means the doubtful result of the operation; still he was prepared for every thing, and was anxious that I should undertake the operation.

Some days afterwards we proceeded to operate. MM. Aronssohn, Segalas D'Etchepare, the celebrated lithotritist, Gaymard, who had twice circumnavigated the globe, the talented Dr. Philip of Lüttich, who had been my assistant in all my operations at Paris, Dr. Deubel of Frankfort, and many other Parisian surgeons whose names I forget, did me the honour of being present. The patient was laid in the horizontal position. Having introduced a full sized catheter into the bladder, I drew back the prepuce firmly, gave the opening the shape of a myrtle leaf placed transversely, and united the transverse wound, having previously bent the glans a little

downwards. One end of each ligature was brought out through the end of the urethra. I next separated a layer of the under surface of the glans as far back as the perforation in the urethra, then pared in the same manner the corona glandis as far as its lateral centre, and finally dissected off as much of the internal layer of the prepuce as made a raw surface exactly adapted to the corresponding surface of the corona glandis, circumference of the defect and glans. After having carefully stopped the bleeding, I drew the prepuce back again over the glans. The pared surfaces fitted each other exactly. Having attached the anterior edge of the skin to the glans by twisted sutures, I divided both layers of the prepuce on each side of the penis, partly to obviate a subsequent phymosis which was here to be expected, but particularly, in order to obtain a free inferior flap which could be attached partly to the glans and partly to the corona glandis; lastly, any drops of urine that should escape might here find exit first.

After the operation was finished the patient was placed in bed, and the penis covered with cold applications night and day. Notwithstanding this, considerable local reaction soon set in. Leeches were applied to the pubis and the inguinal region, and to prevent more certainly the least drop of urine from touching the wound, Mr. Sawyer, a young American surgeon, was kind enough to apply the sucking tube, invented by him, for drawing off the urine constantly from the bladder. (This ingenious apparatus consists of an elastic tube several ells in length, about the thickness of a large catheter, but much more flexible. To the central portion of the tube, a thin bag of gum elastic is attached, which holds about a quart of water, and the neck of which communicates with the canal of the tube by a fine capillary tube. In order to apply the apparatus, the bag is first filled with cold water; the upper end of the tube, which is furnished with a metallic pipe, is fixed into the end of the catheter; the bag is then laid on the edge of the bed, or hung from the board at its side, and the lower

end of the tube is placed in a vessel standing at the foot of the bed, and having its bottom covered with a thin sheet of water. The person applying it sucks for a moment at the lower end, on which, the water ascends from the bag through the capillary tube into the sucking tube, and continues to drop out constantly through the lower half of the latter into the vessel. Immediately afterwards the urine from the bladder follows, and continues to flow off equally with the stream of water.) Messrs. Sawyer, Philip and Deubel emulated each other in careful attention to the patient, whose treatment was conducted by Dr. Aronssohn and myself in common.

After a few days some of the sutures admitted of being removed, and the gradual consolidation of the edges of the wound could be plainly observed. On the fifth day, I looked upon the cure as certain, and thought I might leave Paris for a day without apprehension, and make an excursion into the country. I returned about midnight. As soon as I opened the house door the porter exclaimed, "Monsieur, on vous à demandé plus que vingt fois ce soir, Monsieur S. est mourant." Here was an extremely unpleasant reception at my hotel at night, after having passed the day in delightful society. I immediately hastened to the patient: I found him, certainly, not dying, but what a change was here since morning! The penis was as thick as Dupuytren's speculum vaginæ, its colour dark red, the temperature burning hot, the patient's face swelled, his eyes sparkling and occasionally fixed, the pulse hard and full, and the patient in a state of wild excitement, with occasional raving. A large bleeding from the arm, cold applications to the head, a saline laxative, and local applications of warm aqua saturnina, produced a considerable remission in the symptoms against morning. In spite of the excellent sucking tube, the change in the penis was, unfortunately, to be attributed to a phenomenon well known to me, infiltration of urine, which threatened to destroy every thing again. I therefore removed the rest of the needles during

night, and made an incision at the inferior part of the penis behind the transplanted prepuce, and by making gentle pressure gave exit to a quantity of thin pus mixed with milky urine. I then withdrew the catheter, partly because it had not prevented the infiltration of urine, and partly with the view of diminishing the irritation of the urethra and bladder, and gave directions that the patient should pass urine under water, by dipping the penis in a bucket of warm water.

Next day the local inflamation was so much better, that the success of the cure was no longer exposed to any danger; still, Dr. Aronssohn and I had to contend for some time with the alarming mental excitement of the patient. A continuation of the antiphlogistic treatment, warm baths, &c., ultimately brought down the tone of the nervous system, and in a few weeks the patient was so well as to be able to leave his bed. At my departure the period of the patient's departure was also near. The penis, notwithstanding the extensive cutting during the operation, had quite a natural shape. When the prepuce was drawn back, the under portion of it, by which the loss of substance in the urethra had been repaired, remained attached to the glans; the transplanted flap, however, had become so small, that it looked very like a natural frenum, and though I had attached it to so broad a raw surface on the under side of the glans, it had become free (with the exception of a small string) by the shrinking of the flap. No trace of the former opening was perceptible; when the urine, however, was expelled in a full stream, a slight urinous moisture was observed around a scarely perceptible opening next the flap. I requested Dr. Aronssohn to touch repeatedly the little point which still suppurated with a camel's hair pencil, dipped in concentrated tincture of cantharides. I then left Paris in high spirits, delighted at having witnessed on the spot the brilliant result of the operation.

A considerable time after my return to Berlin, the patient informed me by letter, that the small opening still remained, as

he observed still at the spot a moisture of the skin. I recommended him to continue the application of the tincture, and did not hear from him for a long time, until he informed me last year that he was quite well.

Remarks on the foregoing Methods of Operation.—From all that I have stated here respecting the cure of openings in the anterior part of the male urethra, it follows, that most of my earlier experiments failed, and that the cures according to my recent improved methods, in which I carefully held in view all former unfavourable circumstances and symptoms, though successful, were attended with a train of severe symptoms. These amounted in the case of the young Frenchman, last detailed, to very great danger of life. I was therefore not at all satisfied with these methods operation, which, it is true, had effected the cure of a disease of an extremely unpleasant nature, and hitherto termed incurable; but, at the same time had given rise to very important symptoms. From this period, therefore, I directed my whole attention to the diminishing of these. The infiltration of a few drops of urine into the cellular tissue, was the sole cause to which the frustration of the operation and the violent symptoms were attributable. The twisted suture, with lateral incisions, and detachment of the bridges, certainly obviates this accident completely; still it should be observed, that this method can be applied with advantage only to urethral openings in the middle portion of the penis. I was particularly anxious to discover a method of operation by which the edges of the opening and their immediate circumference, should not be wounded at any point, and exposed to the contact of urine. Where the latter occurrence takes place only at a small point, or where the smallest puncture in the skin is exposed to the contact of urine, an infiltration may take place; generally speaking, capable not only of disturbing the operation, but also of giving rise to the most violent symptoms. The newest method of operation,(to be described immediately,) which has already in one case proved itself as the most eligible, is also adapted to various kinds of

mutilations of the penis. As it requires the least wounding of the parts, it may be tried first, and if it fails, other modes may be selected, according to the indications already given. Superficially considered, it consists in exciting inflammation in the edges by artificial means, and then closing the opening with a ligature. I have tried it successfully for some years in vesico-vaginal and anal fistulæ. The details of these experiments shall be communicated in a future essay.

The Lace-Suture for curing Perforations in the anterior Part of the Urethra.—This is applicable to every part of the anterior portion of the urethra, except where the opening is close behind the glands; but inapplicable in all cases where the perforation in the urethra is not surrounded by healthy and yielding integument. When the edges of the skin are completely deficient, and the tendinous envelope of the corpora cavernosa is only covered with a thin epidermis, it is still less applicable.

1. The edges of the opening are to be pencilled, several times in the day, with concentrated tincture of cantharides. Next morning vesicles are found in and around the opening. The epidermis is to be removed with a fine forceps, and the interior of the opening in particular cleaned with it in a proper manner.

2. Having introduced a short elastic bougie into the urethra, the operator passes a strong double waxed silk thread round the opening, at the distance of one-fourth of an inch from the edge, and nearer the internal than the external opening. As it is not easy to make the needle describe an entire circle at once, it will be necessary to bring it out at least three times, and then to introduce it again through the same puncture. Finally, the needle and the ends of the ligature are brought out through the first puncture. Both ends of the ligature now hang out of the little puncture, while the ring of thread lies buried in the cellular tissue, and surrounds the urethral opening at some distance; externally nothing is to be seen but the ends

of the thread. The application of the suture is entirely regulated by the touch; it must lie deep, but the urethra should not in any case be transfixed with the needle, lest the urine should force its way into the puncture.

3. The ligature is now to be tied. This must be done slowly and gently; the circle is to be made gradually smaller, and finally, the edges are brought into contact so as to obliterate the opening completely. As soon as a second knot has been made to secure the first, the ligature sinks into the cellular tissue, and the only portion of it visible is at the ends which hang out. The bougie is then removed, and neither it nor a catheter introduced, but the patient is directed to pass water in the natural way. The ligature is applied round the opening in the same manner as an artery is tied; where the result is unsuccessful, the same operation may be tried several times in succession.

It is not very easy to apply the suture. When the urethral opening forms a canal, the internal walls of which can be felt externally like a hard string, the suture may be very readily applied after a different manner. The opening, or rather the string which it forms, is to be pressed between the thumb and index finger in a transverse direction, having previously drawn back the skin a little; the hard string now slips through the fingers, and is thrown more forwards. The operator then passes a needle transversely through the integuments and draws it out, leaving the ligature in the wound, and having drawn the integuments somewhat forwards, again transfixes the cellular tissue from the same punctures. In this manner the opening may be surrounded with a ligature in a very easy and simple way. The ends of the thread are then to be tied.

After five or six days, when the ring of thread becomes quite loose, and admits of being drawn out a little, it is to be divided, and the part covered with a piece of adhesive plaister. The catheter is not to be used during the whole treatment, and the patient is to pass water in the natural way. Should a small

fistula remain afer the operation, the application of the lace-suture may be repeated in a few weeks.

This method of operation is indicated in all small fistulæ of the urethra, and I shall always employ it for the future in such cases.

Cure of an Opening in the Central Portion of the Urethra, by the Lace-Suture.—Herr Von Mellin, a Russian marine officer, aged 23, a young man of most amiable disposition, of high mental endowments, and varied talent, was, some years ago, serving on board a Russian ship in the Mediterranean station. The vessel had orders to cruize against the Greek pirates. For a long time no trace of them could be found; at last, one morning as the ship was making way slowly between the rocks, a low Greek pirate vessel was suddenly perceived at a very short distance, which immediately saluted the Russian ship with a discharge of grape shot. While a portion of the Russian crew was engaged on duty, the remainder slept; among the latter was Herr Von Mellin. He was sleeping quietly on the deck, when he was awakened by the roar of the cannon, and the unpleasant sensation of being wounded, and that in a very singular way. As he lay asleep on his back, with the penis in a state of erection, a small, rough, iron, canister shot passed through his trowsers on the right side, tore away the skin of the under part of the penis, and a considerable portion of the urethra, entered the left inguinal region in an oblique direction; ran along the inner part of the left thigh, in the course of the crural artery, as far as the knee joint; and then turning outwards, lodged in the external part of the knee. Owing to the skilful treatment he received from the time of the accident to a later period, the loss of the penis was prevented, and his life preserved, notwithstanding the severity of his wound. Violent inflammation of the penis took place, followed by extensive suppuration, and the wound of the penis did not heal for a considerable time, during which the urine was drawn off with the catheter. The careful and skilful application of strips of adhe-

sive plaster, continued excitement of the edges of the wound, &c., failed in repairing the defect of the urethra by the formation of newly organized substance.

Some time afterwards, when the rest of his wounds were healed, he consulted several distinguished surgeons, who applied caustic to the edges of the opening, and kept a catheter in the bladder, &c., but could not succeed in closing the opening in the urethra, through which the patient now passed water. Several years passed on, during which the patient made water through the false opening. On arriving at St. Peterburgh, the State Physician, Dr. Arndt succeeded in finding the ball, and extracting it by a difficult operation. As the emperor himself took a great deal of interest in this distinguished young officer, Dr. Arndt did me the honour to send the patient to me to Berlin, to see if I could succeed in closing the opening.

Herr Von Mellin's state was as follows:—There was an urethral opening in the centre of the penis, capable of admitting a large sized catheter with ease, and through which he passed water. The anterior portion of the canal remained quite pervious for three years after the receipt of the wound, and when the urine was passed in a full stream, some escaped through it still. At the situation of the opening, judging from the touch, part of the whole circumference of the urethra seemed to be deficient, and to have been replaced by the skin, for it became somewhat puffed during the passage of the urine. Behind the opening the urethra was thickened, indurated, and somewhat narrowed; during erection, according to the patient's statement, the penis was flaccid in the vicinity of the pubis, while the rest of the organ attained a proper degree of firmness. Before the patient left Petersburgh, the stricture had been treated in a very careful and judicious manner; this, however, as is frequently the case, had only produced a transient dilatation. The stream of urine was, however, tolerably large.

The best plan appeared to me to be, first, to attempt to close the opening, and in the next place, to dilate the stricture.

The operation I shall describe briefly at present, as it was performed exactly in the same manner as I have already described. A small portion of a thick bougie was introduced into the urethra, with the view of fixing the penis and closing the internal opening, and the swollen edges of the skin were repeatedly pencilled with concentrated tincture of cantharides. The patient was directed to pass the urine through the catheter alone, which he introduced himself very skilfully.

Next morning, I found a considerable degree of inflammation in and around the opening. The vesicles were removed with the forceps, and the interior of the canal completely cleaned. I then introduced again a short bougie into the urethra, in order to fix the penis during the application of the suture, and having passed a curved needle round the opening, at the distance of a quarter of an inch from the edge, which I was obliged to bring out through the skin three times, (see the mode of operation already detailed,) finally, tied the two double ligatures, and cut off the ends about an inch from the penis, in order to avoid all dragging. The bougie was then removed, and the patient directed to pass water without the catheter.

This slight bloodless operation had given the patient so little pain, that he went about quite like a person in perfect health. There was no subsequent pain of any amount, and there was only a trifling tumefaction of the affected part. The ligature having become loose on the seventh day I removed it, and not a single drop of urine appeared. After the swelling had entirely disappeared and the punctures healed, a small drop of urine occasionally oozed from a small point, the remnant of the former larger opening. I therefore passed a fine needle round the small opening a second time, and enclosed it with a fine ligature.

When I considered Herr Von Mellin completely cured, I exhibited him to my colleagues assembled at the Stosch Medical Society. There was no cicatrix or change in the skin to indicate the situation of the former opening. While Herr Von

Mellin passed water into a vessel, and I strongly drew asunder the inferior surface of the skin to prove that there was no opening, the transmit of a minute quantity of urine, like a drop of dew, was seen in a clear light. I was about to pass a ligature round this secret opening also, but by dilating the narrow passage, and introducing a very fine camel's hair pencil, dipped in concentrated tincture of cantharides, the opening completely closed, and as it did not return, I omitted the operation. To remove the slight narrowing of the posterior part of the urethra, I employed the elastic bougie with the best effects.

Herr Von Mellin is about to use the Bohemian baths this summer for the recovery of his health, and to finish his cure with the baths at Toplitz, as he has not as yet the full use of the wounded thigh.

President Rust, State Medical Counsellors Barez and Kuntzmann, Dr. Romberg and several other physicians who have seen Herr Von Mellin, can attest his cure. He has expressed a wish that his name should be mentioned in this essay.

I shall in the next place communicate my observations and curative experiments on innate fissures of the male urethra, hypospadias and epispadias.

EXPLANATION OF THE PLATES.

Plate 1.

Fig. I. Inferior view of a penis, in which are represented two fistulæ of different sizes. The oblique course of the fistulæ is also pointed out.

a. A narrow fistula in the arterior part of the penis.

b. A larger fistula in the centre of the penis.

Fig. II. View of a penis with an urethral perforation in the vicinity of the scrotum.

a. The perforation in the urethra.

PLATE 2

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Fig 1

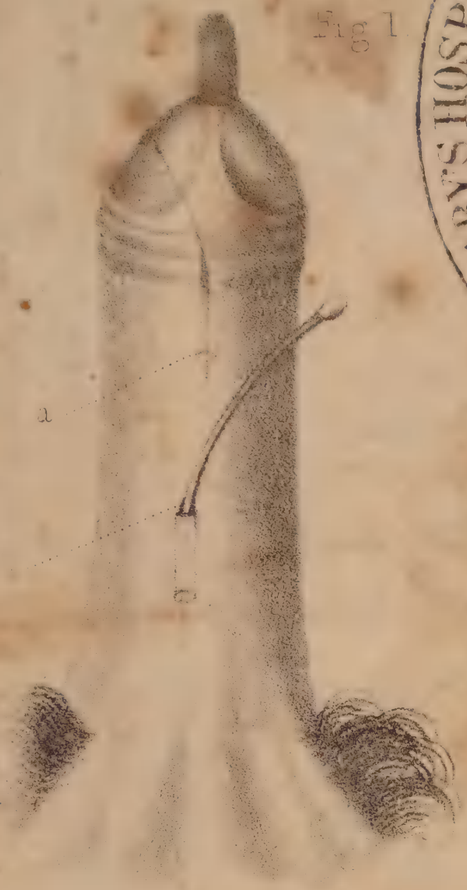


Fig 2.

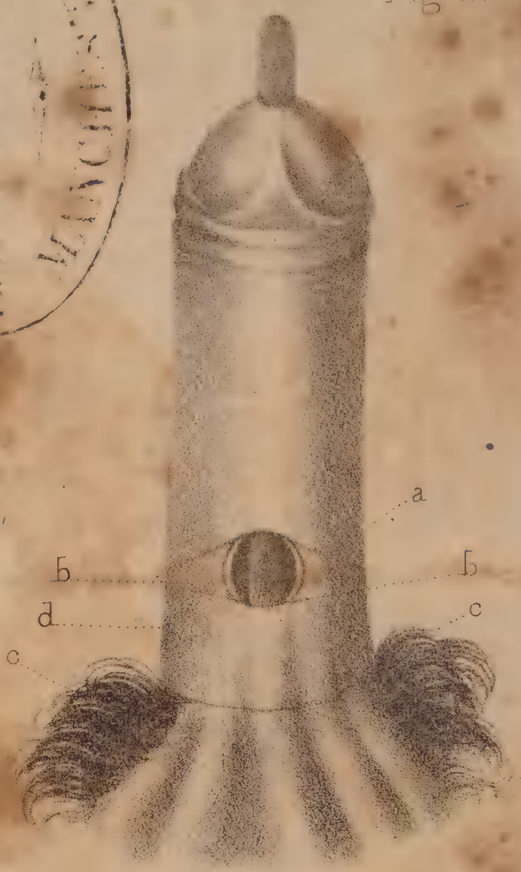
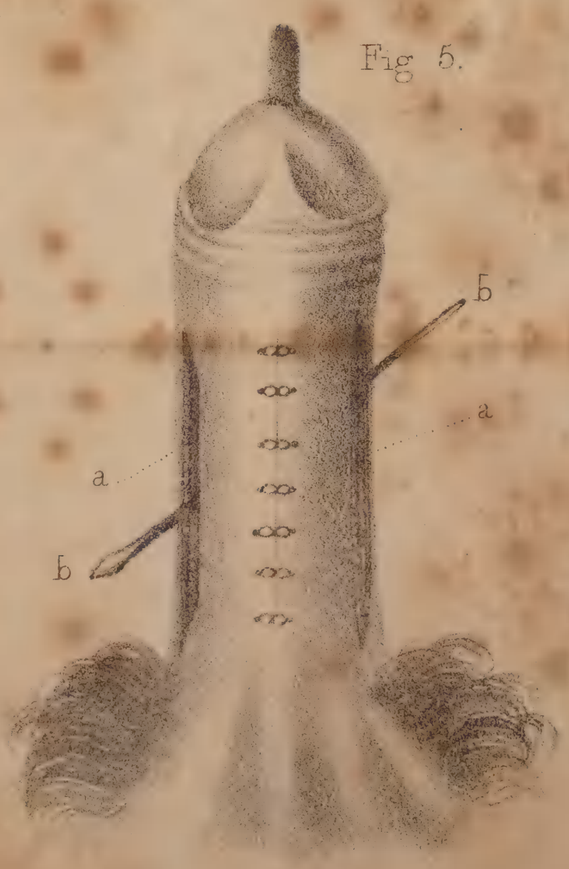


Fig 3

Fig 4

Fig 5.



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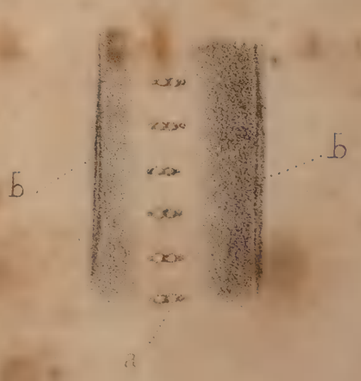

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PLATE 3

Fig 1.

Fig 2.

Fig 4.

Fig 3

Fig 5

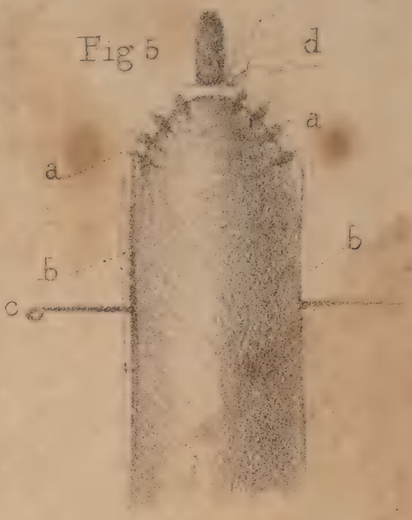


Fig 7.

Fig 6.

Fig 8.

Fig 9.

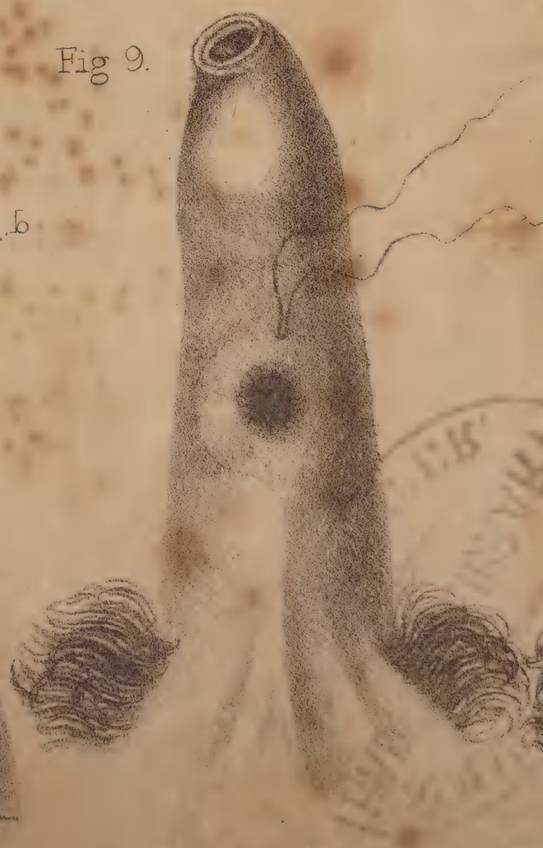
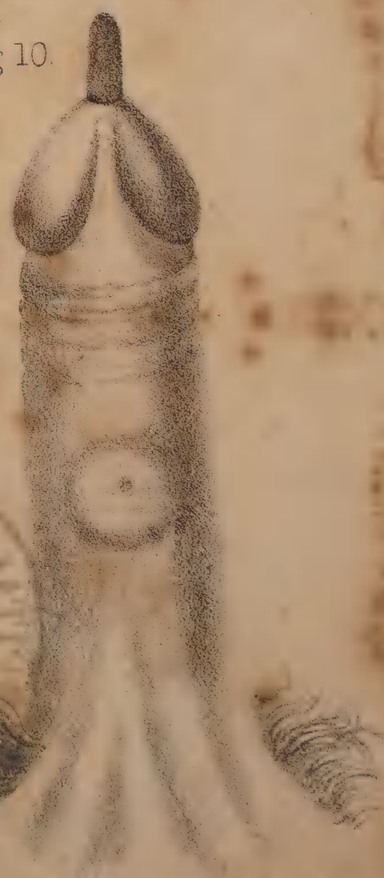


Fig 10.



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bb. The wedges of skin to be dissected out.

c. The lateral incisions through the skin.

d. The bridges of skin to be drawn over the perforation.

Fig. III. Aspect of the posterior part of the penis after the conclusion of the operation.

a. The bridges drawn forward and covering the perforation.

b. The twisted sutures; the ends of the needles cut off.

c. The loss of substance of the skin produced by drawing forwards the bridges.

Fig. IV. Inferior view of the penis, with a circular perforation of the urethra.

a. The opening through which the catheter is visible.

bb. The wedges of skin to be dissected out in removing the edges of the opening.

cc. The lateral incisions.

Fig. V. View of the operation completed.

aa. Seven twisted sutures; the heads and points of the needles cut off.

bb. The lateral incisions.

Fig. VI. Same view.

aa. The lateral incisions separated to a greater distance by the detachment of the bridges.

bb. A probe passed under the bridges.

Plate 2.

Fig. I. A large perforation of the urethra. View before the operation.

aa. Perforation in the urethra.

b. The catheter.

cc. The lateral incisions to facilitate the revolution of the skin.

Fig. 2. Aspect of the parts after the operation, seen from the dorsum of the penis.

a. The perforated opening in the skin pushed up on the dorsum of the penis.

bb. The latteral incisions.

Fig. III. Inferior aspect of the penis, with an opening immediately behind the glans.

a. The opening in the urethra.

bb. The wedges to be dissected out at the sides of the opening.

c. The internal and inferior surface of the prepuce, the epidermis of which is to be dissected off.

d. The flat semicircular wound of the under surface of the penis corresponding to the wound of the prepuce.

e. The catheter.

Fig. IV. Same view after union of the urethral opening alone.

aa. The two interrupted sutures.

bb. The ends of the ligatures hanging out of the urethra.

c. The catheter.

Fig. V. View of the same mode of operation. The prepuce drawn over the glans and attached by twisted sutures.

aa. The sutures.

b. The lateral incision.

c. A probe to shew the complete detachment of the skin.

d. The ends of the ligature which belong to the two interrupted sutures, and by which the opening in the urethra has been closed, hanging out of the urethra.

Fig. VI. Inferior aspect of a penis without a prepuce, in which there is an urethral opening close behind the glans.

a. The opening.

b. The wedge-shaped lateral portion near it dissected out.

c. The annulus of skin formed of the integument of the penis, to be drawn over the opening.

d. The under surface of the glans stripped of epidermis, in the shape of a semicircle.

e. A probe to shew the complete detachment of the under surface of the bridges of skin.

Fig. VII. The same operation. The bridges of skin drawn forwards, and attached to the glans.

aaa. The bridge of skin attached to the glans by twisted sutures.

b. The inferior part of the penis stripped of skin.

c. The ligatures by which the opening in the urethra is closed.

Fig. VIII. View of the upper surface of a penis without any prepuce, on which are seen the ends of the incisions, by which the bridge of skin is formed.

Fig. IX. A penis, with a large circular perforation.

a. The opening.

b. The point where the needle is introduced.

cc. The ligatures for tying the opening.

d. Delineation of the track in which the needle passes under the skin. The circular ligature previous to being tied. Representation of the method of operating employed in the case of Herr von Mellin. *

Art. XIX.—*Account of some Particulars in the Anatomy of the Fifth Nerve in the Human Subject, and in Fish, not hitherto observed.* By B. Alcock, M. D.

[Communicated to the British Association at Bristol, August, 1836.]

ORIGIN OF THE FIFTH NERVE IN MAN.

The cerebral connexion of the fifth nerve in man having lately occupied my attention, I beg to offer to the section some re-

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* *Note by the Translator.*—The reader will perceive some discrepancies between the foregoing explanation and the lithographic illustrations. As these inaccuracies, however, exist in the original plates, the translator has not considered it his duty to interfere with them. They are besides of trifling importance, and can be easily rectified.

markable particulars in it, not hitherto, I believe, observed. First, I have traced both packets of the nerve to the same point of attachment; it is situate behind the middle crus of the cerebellum, in the angle formed by the three crura, behind and beneath the middle, above the inferior, and before the superior. Secondly, the point of attachment presents, when the surrounding structure has been carefully pushed away from it, a small eminence, which is imbedded in the substance forming the floor of the fourth ventricle, separated from the interior of the chamber by a thin lamina only. Thirdly, I have found beside the fasciculus discovered by Rolando, which descends into the inferior crus of the cerebellum, and connects the attachment of the nerve with the posterior column of the spinal cord, a second and smaller fasciculus, which desends from the point of attachment, behind, and within the inferior crus, and between the posterior pyramids, enters the posterior fissure of the bulb and cord, and can be traced for some way along the back of the anterior column of the cord, into which it seems ultimately continued. These facts will, if verified, be deemed interesting. It is contrary to our general impression, that the two portions of the nerve, having, as we believe, distinct functions, that they should be attached to the same point of the cerebro-spinal axis; but the existence of a cord of communication with the anterior as well as with the posterior column of the spinal cord, removes any difficulty which might be created by the distinction in function between the two; and indeed, such a connexion might have been anticipated, as well from the compound nature of the nerve as from the previous establishment of one with the posterior column. The points being new, I have felt anxious to communicate them.

FIFTH NERVE IN FISH.

The section is aware that in the human subject and the other mammalia, the fifth nerve presents the remarkable disposition of having its non-ganglionic portion associated altogether with

one division of its ganglionic; from which union, results the inferior maxillary nerve. The same appears to prevail throughout birds and reptiles, so far as my experience extends; but in fish, to which my attention has been lately directed with reference to this point, I have found it otherwise, for in them the non-ganglionic portion is distributed along with several of the primary branches of the ganglionic, presenting a most remarkable, and it may prove, important difference from the distribution of the nerve in the other classes.

In the Ray, (Raia Clavata,) which I select as an instance of the cartilaginous fish, the fifth consists originally of three parts, one non-ganglionic, and two ganglionic. The two ganglionic portions have each a distinct ganglion: the first or anterior giving rise to the first and second, the second to the third divisions of the nerve. The non-ganglionic arises or is attached above, and between the ganglionic fasciculi, and enters into, or emerges from, a lateral convolution of the cerebellum: at first it is distinct from both the others; but presently it is united upon its under side to the second or posterior ganglionic fasciculus, and soon after divides into two branches, one of which accompanies the first, and the other the second divisions of the anterior ganglionic fasciculus. Thus in this instance each division of the nerve consists of a branch of each, as well one from the non-ganglionic as from the ganglionic portions of the nerve.

In the Cod, (Gadus Morrhua,) an instance of the osseous class, the original constitution of the nerve is more complicated than in the ray, but its disposition, in the particular in question, is even more positive and satisfactory. In it also there are two original ganglionic portions, provided, each, with a ganglion as in the other; but the ganglia are connected together, by the branches arising from them. There are also two non-ganglionic portions; the latter are situate, at their attachment to the brain, as in the ray, above the ganglionic, and they hold the same relation through a great part of their course, being also

quite distinct, and appearing simply to accompany, but not to be identified with the divisions of the ganglionic. Six divisions arise from the ganglia, with four of which the branches of the non-ganglionic portions are associated; those of the anterior with the first, second, and third, viz. the ophthalmic, the common trunk of the superior and inferior maxillaries, and a branch corresponding to the temporo-malar of other animals; and the posterior with the fourth—the opercular.

The fifth, the pterygo-dorsal nerve, appears not to be accompanied by any part of the non-ganglionic portion; nor is the sixth the palatine. And when it is recollected that the existence of ganglia upon the spinal nerves of fish is a question even still in dispute, and that the pterygo-dorsal branch is one which does not exist in other animals, it seems likely that it is intended to supply the surface of the body or the fins of the animal with the endowment of a ganglionic nerve, which in other instances is derived from the nerves of the spine. It is also to be remarked, that in the pectoral and abdominal fins, at least, the branches derived from the pterygo-dorsal divisions are distributed not generally to the fins, but to the anterior part of them, and thus to the part likely to be first exposed to the contact of objects: at all events this peculiar distribution has not been effected without a purpose, and, if the account, which I have given of the source of the pterygo-dorsal division, be correct, it reduces us to one of two alternatives, either that that nerve supplies to the parts to which it is distributed a faculty, which otherwise they derive from the spinal nerves, or that the fifth nerve is endowed with one, with which the spinal nerves are not, and which is thus extended to the parts in question. I have not discovered any account of the arrangement described in any authority. In Tab. XXXIV. of the work of Monro on Fish, may be seen a delineation of the fifth nerve in the Skate, in which the ophthalmic division is represented composed of two distinct portions, and in Tab. XXXVII. the compound character of the same division is represented in a

further stage of its course. But Monro evidently had no idea of the nature of those distinct portions, nor of the constitution of the nerve itself, and he merely delineated, so far as has been mentioned, the fact; for first he states at p. 44, "that ganglia are wanting in their nerves (of fish,) and consequently, he could not distinguish between a ganglionic and non-ganglionic portion; and secondly, he represents the compound constitution of one division only. Scarpa, and after him Desmoulins, also mention the double constitution of the ophthalmic division of the fifth in fish, but they make no allusion whatever to a difference in the nature of the two portions, and they ascribe such constitution only to the ophthalmic division. I have found the same disposition in the torpedo as in the common ray, with this difference that the part of the non-ganglionic portion, which accompanies the third division of the ganglionic, and which is united to that division in the common ray, is distinct from it in the torpedo, and does not enter the electric apparatus in which the other is consumed. For the opportunity of ascertaining the disposition of the nerve in the torpedo, I am indebted to Dr. Houston, Conservator of the Museum of the College of Surgeons. In the turbot I have found one analogous to that in the cod, though not strictly similar; it has not I think a pterygo-dorsal division.

So far as I have been able as yet to determine the distribution of those non-ganglionic portions, they seem to me to supply rather, parts which are not muscular. Should the account which I have given be confirmed, it must open again the question of the properties of the non-ganglionic portion of the fifth nerve in the higher classes; so far as it can be considered identical with that in fish; and I have offered the several particulars, which I have stated, that they may be confirmed or disproved.

It is my intention to discuss the last question started more at large in a future number of the Cyclopædia of Anatomy and Physiology, in which I hope also to give sketches of the peculiarities in the anatomy of the fifth nerve, which have been described.

BIBLIOGRAPHIC NOTICES.

Report on the State of the Library, Museum, and Record Room of the Medical Department of the Army. May, 1836.

Every one acquainted with the present state of the medical department of the army, and who has had opportunities of comparing what has been with what is, will be ready to acknowledge, that this branch of the public service has progressed more rapidly than even the most sanguine could have anticipated.

It is needless to dwell upon the vast importance of rendering our army surgeons capable of discharging the difficult and multifarious duties committed to their charge. As a matter of mere government economy, it behoves our rulers to provide the best medical aid for the sick. Take a private soldier and calculate what he has cost from the time he was enlisted until he became perfect in discipline, and a person unaccustomed to such matters will feel surprised when told, how much the expense of the manufacture has added to the price of the raw material. Every good soldier that dies is a clear loss to the country of at least fifty pounds; for this sum must be expended on a recruit before he can be made capable of supplying the place of the deceased. Again, suppose the soldier dies in a distant colony, say the East Indies, this sum will be nearly doubled, for we must add the expense of the voyage out. Those who preside over the military affairs of the empire have long ago hit upon an excellent expedient for saving the expense of the home voyage, for the period of service in the East is so long, that few return. In war the value of the soldier's life to his country cannot be too highly estimated, and consequently in war the medical department of the army occupies a most prominent position.

It is now ascertained that, even in the most sanguinary campaign, the losses to the effective strength caused by disease much exceed those inflicted by the sword of the enemy. Without dwelling further upon a truth so obvious, and without expatiating on the varied and difficult duties the army surgeon has

to perform, in so many different climates, and under so many trying circumstances, we may be permitted to express a doubt, whether any practitioner in civil life has such need of extensive knowledge, and requires to be master of such resources. With this impression strongly fixed upon his mind, Sir Jas. M'Grigor has for many years strenuously endeavoured to introduce a better system of education among the candidates for the rank of assistant surgeons, and he has at the same time left no means untried to excite the whole body of those already employed to the most active and diligent application of their knowledge. Sir James has been eminently successful in producing a degree of useful emulation among the officers, whether on service at home, or abroad; and in furtherance of this object he has wisely sought to combine their efforts, so as to confer lasting credit upon themselves, and benefit on the profession in general.

This he has effected, not merely by prescribing a regular system of returns connected with the routine duty of each regiment; returns embracing much that is valuable in practical, pathological, and statistical medicine and surgery, but also by encouraging all to extend their inquiries in such a manner as to elucidate a great deal hitherto obscure concerning the various endemic diseases met with in the numerous colonies of Great Britain. In order to concentrate the mass of information thus acquired, in order to render it available, it was necessary to adopt the system of centralization, by creating a depot, to which each officer might send his contributions. Accordingly, under the auspices of Sir James M'Grigor, the medical department of the British army has lately witnessed the rapid consolidation and growth of an institution, where all its records are forwarded, and its most valuable labours are brought together and systematically arranged. How useful these records may prove can be judged of by the works of Guthrie, Marshall, Hennen, and other late writers. It is now Sir James's object to deposit all those precious stores in a building, to be specially erected for that purpose in London, where all the officers coming to town can meet, and where they can hold their annual and stated councils. Here are to be deposited the splendid collection of natural history, comparative and morbid anatomy, and the excellent library which the zeal and ability of the surgeons of the army have already accumulated: and here the young and inexperienced will enjoy the advantage of having their emulation stimulated by examining what has been done by their seniors and predecessors. It would be an injustice to Sir James M'Grigor, were we to refrain from expressing the pleasure we have felt in perusing the Report of the Army Medical Officers' Benevolent

and Assurance Societies,—societies whose institution has already proved a blessing to many who would have been otherwise comparatively destitute. It is a maxim of the British constitution, that, its subjects in becoming soldiers do not cease to be citizens; and we prefer the system which provides for the widows and orphans of the deserving and of the brave, to that narrower and more economical policy of some foreign states, which guards against such destitution by almost enforcing the celibacy of military men.

Manuale Practico per la Cura degli apparentemente morti premessevi alcune idee generali di Polizia Medica per la tutela della vita negli Asfittici, opera de Pietro Manni. Naples, 1835, 4th edition, 8vo., pp. 329.

This work of Professor Manni on apparent death, and the recovery of asphyxiated persons, is well worthy of the attention of all English writers and lecturers on medical jurisprudence. Professor Manni's manual is complete in all its parts, and contains not only a vast fund of erudition, but the results of his own extensive experience. The section devoted to the consideration of the best means of applying galvanism in cases of asphyxia contains much that is original, and may be usefully consulted by all those who feel an interest in this important subject. The Professor enters particularly into detail concerning the apparatus most suitable for this purpose, and gives a description of a galvanic arrangement which seems admirably well constructed, combining the advantages of being very portable, easily worked, and sufficiently energetic in its action.

An introductory Lecture delivered to the Mathematical Class at the Royal School of Medicine and Surgery at Birmingham. By the Rev. William M. Lawson, M. A.

We have perused this lecture with much pleasure, and have been induced to notice it for the purpose of drawing the attention of our readers to the great improvement daily taking place in the system of medical education. Formerly all who aspired to reputation, were constrained to study at a foreign university, Paris, Pavia, or Leyden. During the last century the schools of Scotland earned for themselves an European celebrity, and Edinburgh enjoys the merit of having not only kept her sons at

home, but attracted foreigners to her schools of medicine and surgery. More lately Dublin has become a much frequented school for students in all branches of professional education, and we trust the numerous advantages our teachers possess may long render their instructions useful and popular. Within a few years, much within the period of our own memory, provincial towns in England, Scotland, and Ireland have founded schools, whose establishment has been attended with consequences the most useful. We have been always against centralization, and have always contended that a much more useful system of instruction is likely to arise, when several institutions are founded for the same purpose in different parts of the kingdom, than when the metropolis possesses a monopoly. These considerations induce us to hail with pleasure the manifestly growing, the deservedly growing reputation of Birmingham, Bristol, and Norwich. We regard all these schools without jealousy, and feel an inward satisfaction in thinking how many learned and experienced men are simultaneously occupied in directing the studies of those to whom in a few years will be intrusted the lives and health of every inhabitant of Great Britain. We hope that no ill judged system of legislative interference will cramp this healthy multiplication of the sources of knowledge.

The Royal School of Medicine and Surgery at Birmingham seems to be conducted in a most judicious and spirited manner, and no care has been spared to render the education of students complete in all its parts.

Researches into the Physical History of Mankind. By James Cowles Prichard, M. D. Third edition.

The first edition of this celebrated work was published in 1813; the second, and the present, have been almost entirely written anew, and every topic reconsidered, with the advantage of such additional information as the author had acquired during the interval elapsed since the first appearance of his work. No book, published of late years in England, has obtained a greater reputation on the Continent than these Researches, which are quoted by all as a most valuable contribution to the physical history of mankind, a science Dr. Prichard was the first to treat in a systematic form; for strange to say, the comparative physiology and psychology of different races of mankind, had never been made expressly the object of inquiry until the publication of his treatise. Blumenbach, indeed, had, a few years previously, worked out one part of this extensive

field, and had brought a great mass of information and original observations to bear on the comparative anatomy of human races, particularly as distinguished from each other by the form of the skull: a subject on which we ourselves had the pleasure of hearing that celebrated Professor at Göettingen, in an admirable course of lectures delivered in 1817. Still, we repeat it, Doctor Prichard's was the first work laying claim to the merit of treating the whole subject of the natural history of mankind, in a physiological and psychological point of view combined. One obvious question, long since indeed, engaged the attention of inquirers, viz., the unity or diversity of species in all human races. Blumenbach, Prichard, and Lawrence advocate the opinion of one original stock; Cuvier, Bory de St. Vincent, Virey, Desmoulins, Rudolphi, Humboldt, Spix, and Martius all maintain an original diversity of species; this latter opinion is supported by nearly all continental writers of physiology or travels. The sanction of authority is consequently more in favour of a diversity of species. The matter, however, is of too great importance, to allow us to adopt implicity the opinion of the majority; we must here appeal to the facts and arguments brought forward by each party, and having done so, and having studied both sides of the question with great care and perfect impartiality, we are constrained to throw the scanty weight of our opinion into the same scale with that of Doctor Prichard. Did space permit, we should gladly indulge our readers with some extracts from this admirable treatise; at present, we can do no more than recommend it in general terms, as one of the most interesting and learned works that has been printed in England for many years.

Facts and Cases in Obstetric Medicine, with Observations on some of the most important Diseases incidental to Females. By J. T. Ingleby, Lecturer on Midwifery at the Royal School of Medicine, Birmingham. London: Longman, &c.

Mr. Ingleby's former work on Uterine Hæmorrhage has acquired a considerable degree of reputation, and we are certain, that the facts and cases now published will be found equally deserving of praise. As Mr. Ingleby has from time to time contributed valuable papers to this Journal, we feel scarcely authorized to deal with him as with a stranger; were we so to do, we fear that our unqualified approbation of his work might be misconstrued, and we should incur the risk of being

censured for bestowing commendation upon one, whom we consider almost as a coadjutor. We must, therefore, request Mr. Ingleby to pardon us for not entering into an analysis of his treatise, a labour we would have gladly undertaken under other circumstances; for it is always a pleasing task to develope the views and practice of a lucid and original writer. Even if we felt justified in giving free scope to our inclination, in following Mr. Ingleby through the various important and interesting topics he has handled with such ability, we could not have done so in our present number, which, thanks to the labours of our friends, contains scarcely room enough for the original articles. Our readers must indemnify themselves for this omission by reading the work itself, the design and scope of which will be best understood by its chapter of contents.

Sect. I.—On puerperal convulsions.

Sect. II.—On malposition of the uterus, ovaria, bladder, and urethra, both in the impregnated and unimpregnated state, in connexion with retention of urine.

Sect. III.—On obstructions in the soft parts to the progress of labour.

Sect. IV.—On the induction of premature labour in cases of organic disease.

Sect. V.—On laceration of the uterus and vagina.

Sect. VI.—On inversion of the uterus.

Sect. VII.—On the signs and symptoms of pregnancy—their obscure and deceptive characters—their complication with disease, and the signs which denote the extinction of life in the fœtus.

Appendix to Section VI.

Observations on the principal Medical Institutions and Practice of France, Italy and Germany, with Notices of the Universities, &c. &c. By Edwin Lee, formerly House Surgeon of George's Hospital, &c. London: Churchill, 1835.

This is a very useful volume and contains much valuable information. The following passages relative to practical subjects will at once serve as specimens, and will be the means of communicating to our readers several excellent hints concerning the treatment of disease.

"*Strictures of the urethra* are treated in continental hospitals by rest in bed, a catheter being kept in the bladder, and its size in-

creased by degrees. In cases of retention of urine, where great difficulty existed in introducing a catheter, M. Dupuytren did not persevere in the attempt, but adopted antiphlogistic measures; passed a bougie as far as the impediment, and fixed it there. An abundant mucous secretion was produced, which allowed the bougie to be passed a little further into the urethra at the expiration of a few hours. It was again fixed in its new position, and thus proceeding by degrees, did not fail to arrive at the bladder."—p. 34.

"*Strumous ophthalmia* was considered by M. Dupuytren to depend on inflammation of the retina, and was treated by the internal administration of belladonna, combined with other means indicated by the symptoms. From three to eight grains of the powder, or from one to three grains of the extract of belladonna, were divided into six doses: the patient took one of these every two hours; to prevent narcotism, either general or local, Seltzer water was usually administered at the same time."—p. 40.

"*In prolapsus ani*, M. Dupuytren excised two, three, or more folds of the skin on the margin of the anus, on either side. A similar operation was recommended by Mr. Hey. No dressing is required, and the recurrence of the disease is effectually prevented.

"Fissures at the margin of the anus may be divided into three kinds:—1st, those external to the sphincter, which are not very painful, and do not occasion spasmodic contraction; 2nd, those situated within the sphincter, affecting principally the mucous membrane—this kind causes tenesmus, and great pain, especially on the patient's going to stool; 3rd, those placed on the same level as the sphincter, are more serious and painful than the other kinds, which may generally be cured by simple dressings, emollient lotions, and sedative applications; whereas this variety requires the division of the sphincter on the fissure. M. Dupuytren was in the habit of prescribing an ointment composed of extract of belladonna and acetate of lead, of each a dram, to an ounce of lard, for alleviating the pain in these diseases."—p. 41.

"*The following are Biett's opinions with respect to the use of baths in cutaneous diseases.*—Simple tepid baths are most beneficial in the dry scaly forms, though only as an accessory means; their efficacy is less marked in the pustular varieties: they are serviceable in vesicular affections when the inflammation begins to decrease, and may be used with advantage in impetiginous affections where incrustations have succeeded to the pustules.

"Alkaline baths are efficacious in the papular and dry scaly forms, and in the impetiginous and tubercular varieties. An alkaline bath may be formed by dissolving in a simple bath from half a pound to a pound of carbonate of soda.

"Sulphur baths are most useful in the decline of vesicular affections: they are less useful than alkaline baths in the chronic stage of psora, and if used in the inflammatory stage the symptoms are

aggravated. Sulphurous baths are composed of two ounces of diluted sulphuric acid and eight ounces of hydro-sulphuret of potass added to each bath.

"Acid baths may be made by adding to each from four to eight ounces of hydro-chloric acid; they are mostly applicable in dry scaly eruptions."—p. 69.

"*M. Baudelocque speaks highly of the efficacy of sulphur baths in the treatment of chorea*, this remedy often succeeding after the failure of other means. The proportion of girls affected by this disease is very much larger than that of boys, and it is found to affect the left side more frequently than the right; post-mortem inspections throw no light on its nature. M. Baudelocque has not observed that the disease is ever propagated by imitation among children placed in the same ward."—p. 76.

"*Paralysis of the bladder, and vesical catarrh*, in elderly people are treated in the following manner:—a stream of cold water flows from a reservoir fixed near the ceiling, through an elastic gum tube, having stop-cocks, and terminating in a silver catheter formed into a double tube by a central position. The patient being in the recumbent position and the catheter introduced, the water passes into the bladder by one side and out by another. A continued stream of water through the bladder is thus kept up for about ten minutes, and repeated every second or third day: the quantity of water passing into the bladder may be regulated by the stop-cock, so as to prevent undue distention. The beneficial effects of the method are attributed to the clearing away of the accumulated mucus, and to the tonic action of the cold water upon the bladder."—pp. 79, 80.

"Among the patients was also *an old man, with chronic entropion:* by touching the central point of skin beneath the lower eyelids, with concentrated sulphuric acid, for three or four minutes, an almost instantaneous rectification of the position of the previously inverted lids was produced. In these cases, after the separation of the slough, the skin becomes contracted, while cicatrization takes place, and the benefit is permanent."—p. 161.

"*The syphilitic wards* in the Charité at Berlin are placed at the top of the house, because while under treatment patients are not allowed to leave, nor are their friends permitted to visit them. They are clean and well-ventilated, containing fifty beds for women, and thirty for men. The treatment differs from that in use elsewhere, and consists principally in rigid abstinence: bread, soup, and milk, are the only articles of food allowed; and of these each patient receives only a quarter of the ordinary portion given to the other patients. Mercury is never employed; the usual remedies being solution of the neutral salts, especially sulphate of magnesia, and sarsaparilla. Where mercury had been given previous to the patient's admission, sulphur is administered for a few days. The applications to sores are emollient, slightly stimulating, or caustic, according to circumstances. The usual duration of the treatment is

from three to four weeks : it is seldom that secondary symptoms make their appearance, and when they occur they are not of a serious nature. I did not observe any of those deformities of the face so commonly seen in similar institutions, nor more than three or four cases with cutaneous eruption;—in fact, most of the patients appeared to be in a thriving condition, notwithstanding their meagre diet."—pp. 163, 164.

SCIENTIFIC INTELLIGENCE.

Considerations respecting a New Power which acts in the Formation of Animal Bodies, by M. Berzelius.—When new compounds are formed in unorganized substances in consequence of action between different bodies, it is the result of the mutual tendency of these bodies to comply, in a more perfect way, with their affinities. On the one hand, those substances whose affinities are the strongest combine; and, on the other, those which have the weaker affinities are expelled. Previous to the year 1800, it was not supposed that any other determinate causes of these phenomena existed, than the power of this affinity itself, along with heat, and, in some circumstances, light. At that date the influence of electricity was detected; and shortly afterwards we were led to confound the electrical agency over bodies with the chemical, and to consider affinity as nothing more than the manifestations of opposite electricities, heightened by light and heat. But still, even this system supplied no other means of explaining the origin of new compounds than the supposition that, by the approximation of bodies thus put into contact, the electrical influence succeeded in more completely neutralizing them.

Starting with these views, which are deduced from the effects which occur in unorganized bodies, and then studying the chemical actions which organized bodies present, we observe that in the organs of these latter the most different kinds of products are elaborated, notwithstanding that the matter whence they all proceed consists in general only of one identical liquid, circulating in the vessels with more or less velocity. The vessels of the animal body, for example, without interruption, receive blood from the heart, and, nevertheless, at their extremities secrete milk, bile, &c. without the admission of any other liquid which is capable, in the way of double affinity, of affecting any decomposition whatever. There is clearly here a fact, of which the science of unorganized matter can give no explanation.

At this epoch M. Kirckhoff discovered that starch, dissolved in a diluted acid, is transformed, at a certain temperature, first into gum, and then into sugar. It was then inquired, according to the prevailing views with respect to effects of this kind, what that substance was which the acid had taken from the starch in reducing it into sugar; but it was found that no gas had escaped, as the acid

reappeared, by means of alkalies, in its original quantity; that no new combination had been formed; and that the liquid contained nothing else than sugar, in quantity equal, and even superior, to the quantity of the starch employed. The cause, then, of this change was as problematical as that of the secretions in organized bodies.

M. Thénard soon afterwards discovered the peroxide of hydrogen, a liquid whose elements are very feebly retained together. Upon this substance acids produce no change; but alkalies, on the contrary, occasion a tendency to decomposition, a species, in short, of fermentation, in which, of consequence, there is a separation of oxygen, and water remains behind. But what is peculiarly interesting is, that the same effect is produced not only by the action of such bodies as are soluble in this liquid, but also of various solid bodies, some organized and others unorganized; as, for example, by the peroxide of manganese, silver, platina, gold, and even by the fibrine of blood. The substance which produces the decomposition undergoes no alteration; nor does it become an element of the new compound, and therefore it operates by an inherent power, which, though unknown as to its essence, is, nevertheless, demonstrated by its effects.

Shortly before this discovery of M. Thénard, Sir H. Davy had noticed a phenomenon, the connexion of which with the preceding was not immediately recognized. He had proved that platinum, heated to a certain extent, and brought into contact with a mixture of the vapour of alcohol, or of ether and atmospheric air, possessed the power of producing the combination of these bodies, whilst other substances, such as gold and silver, had not this property.

A short time after this, Mr. E. Davy found that a preparation of platinum in a state of extreme mechanical division had the power, at ordinary temperatures, and after being moistened with alcohol, of becoming incandescent by the combustion of alcohol, at the same time changing this liquid by oxidation into acetic acid.

After this followed the discovery of Döbereiner, which was the most important of them all. He demonstrated the property which spongy platinum has of setting fire spontaneously to a current of hydrogen gas projected into atmospheric air; a phenomenon which the researches of Thénard and Dulong extended to many other bodies, both simple and compound, but with this restriction, that, whilst platinum, iridium, and some other metals of similar character, acted at temperatures below 32° Fahr., those other bodies, such as gold, and still more silver, required much higher temperatures, and glass a heat of 300° or more.

Thus, this property, which at first was considered as acting in a way that was altogether singular, appeared to be a general property, though acting differently in relation to different bodies; and it became possible to deduce from it certain applications. We now know, for example, that in the act of fermentation, in the transformation of sugar into alcohol and carbonic acid, the change which is effected by the insoluble substance which is called *ferment* or *yeast*, and which may be replaced, though with less certainty, by animal fibrine, by

albumen, by cheesy matters, &c. &c., cannot be explained by any chemical action between sugar and yeast, and that no phenomenon in unorganized bodies approaches it so nearly as the action of platinum, of silver, and of fibrine, in the decomposition of the peroxide of hydrogen into oxygen and water. It was, therefore, only natural to suppose that the mode of acting of yeast was analogous.

The transformation of starch into sugar, by means of sulphuric acid, had not hitherto been arranged and connected with the preceding facts; nevertheless, the discovery of *diastase*, a substance which acts upon starch in a similar manner, but with much greater energy, directed attention to this analogy; and the parallel was completely demonstrated to our satisfaction by the ingenious researches of M. Mitscherlich regarding the formation of ether. Among the many theories respecting the formation of ether, one, as well known, made the power of the sulphuric acid to transform the alcohol into ether to depend upon its power of combining with water, admitting that the alcohol, considered as a compound of one atom of etherine (C^4H^3) and two atoms of water, was converted into ether, by yielding the half of its water to the acid. This theory, as simple as it was ingenious, was in perfect harmony with our knowledge of the actions of the affinities of bodies; but, notwithstanding, it did not explain why other non-acidulous bodies, as strongly disposed for water, did not also produce ether. The researches of M. Mitscherlich now prove that sulphuric acid, properly diluted, and taken at such a temperature that the refrigeration produced by the addition of the alcohol may compensate for the heat which is produced by the mixture, decomposed the former into ether and water, both of which, owing to the temperature surpassing the boiling point of water, separated themselves by distillation from the mass, and presented, when completely condensed, a mixture of the same weight with that of the alcohol employed. The method of operating in this experiment, as well as the fact of the distillation of the water conjointly with the alcohol, was, it is true, known before M. Mitscherlich, but to him belonged the merit of foreseeing the consequences. In a word, he demonstrated, that at this temperature the sulphuric acid acted upon the alcohol, in virtue of the same power which determines the action of alkalies upon oxygenated water, since the water, separating itself entirely from the mixture, had not obeyed any affinity for the acid; and he hence concluded, that the action of the sulphuric acid and the diastase upon starch, whence resulted sugar, must be of the same nature.

It is proved, therefore, that many substances, simple and compound, solid and in a state of solution, possess the power of exercising upon compound bodies an influence essentially distinct from chemical affinity, an influence which consists in the production of a displacement, and a new arrangement of their elements, without their directly and necessarily participating in it, some special cases only excepted. Assuredly such a power, which is capable of effecting chemical reactions in unorganized substances, as well as in organized bodies, although still too little known to be accurately explained, must play a far more important part throughout nature than we have hither-

to been led to suppose. In defining it a new power, I am far from wishing to deny that some connexion exists between its influences and the electro-chemical ones, with which we are more familiar; on the contrary, I am very much disposed to recognize in it a peculiar manifestation of these same influences; but notwithstanding, so long as we have not ascertained the real nature of this power, it will be more simple, so far as regards our future researches, to consider it as independent, and to confer upon it, for the facility of comprehension, a particular name. Accordingly, I shall designate it, thereby following a well known chemical etymology, the *catalytic power* of bodies; and the decomposition it produces I shall call *catalysis*, in the same way as we have designated by the term *analysis*, the separation of the elements of a compound, by means of the ordinary chemical affinities. This power seems *definitely* to consist, in a faculty of bodies, by their simple presence and without any chemical participation, to rouse up the play of certain affinities which at that temperature remained inactive, so as to determine, in consequence of a new arrangement of the elements of the compound, a new state of perfect electro-chemical neutralization. As this agent acts generally in a manner analogous to heat, it may be demanded, if being differently graduated, sometimes by a different mode of using the same catalytic body, sometimes by the introduction of different catalytic bodies in the same liquid, it would produce, as we often see in the action of different temperatures, different catalytic products; and if, on the other hand, the catalytic power of a body can exert itself upon a great number of compound bodies, or whether, as our experiments appear to indicate, only upon certain bodies, to the exception of others? But in the present state of our knowledge it is impossible to decide these questions, as well as many others which might be agitated on the subject; and their solution must be left for future research. It is sufficient, for the present, to have demonstrated the existence of this power by a number of examples; which power, as now explained, sheds a light altogether new upon chemical agency in organized bodies. We shall give only one example: round the eye of the potato we find a portion of *diastase* accumulated, which is totally wanting in the tuber itself, and in the developed germ: in this point we recognize a catalytic centre of action, in which the insoluble starch of the tuber is changed into gum and sugar; and this portion of the potato becomes the secreting organ of those soluble substances, which go to form the juices of the nascent germ. It is not at all likely that the action now mentioned should be the only one of its kind in vegetable life; on the other hand, we may decidedly presume that in vegetables, as well as in the animal body, a thousand catalytic effects take place between the solids and the fluids, whence really result the great number of different chemical compounds, whose production at the expense of the same physical fluid which we call blood, or vegetable juice, is to be explained by no other known cause.—*Translated from Poggendorf's Annalen, for the October Number of the Edinburgh New Philosophical Journal conducted by Professor Jameson.*

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XX.—*Researches on the Symptoms and Diagnosis of Aneurismal and other Tumours in the Cavity of the Thorax.* By GEORGE GREENE, M. D., Fellow of the College of Physicians, one of the Medical Inspectors of the House of Industry, and Lecturer on the Practice of Medicine in the Richmond Hospital School of Medicine and Surgery.

THE study of medicine is pursued at the present day by a method widely different from that followed by its cultivators during the last and preceding centuries. Were we desired to point out in what this difference consists, we would say, in the attempt to refer all those derangements of vital actions termed symptoms, to so many lesions of particular structures. To such an extent has this been carried, that there is scarcely a disease to which the above method of investigation has not been extended; and it must be admitted, that since its adoption much of the obscurity which formerly prevailed, particularly with respect to affections of internal organs, has been removed.

Pathological anatomy, however, has not yet reached such a degree of perfection as to enable us to predict, in all cases with confidence, that an organic lesion exists, from the observations during life of a particular series of symptoms; a remark fully borne out by the most zealous pathologist of modern times,—"Ce que vous voyez," says M. Andral, "sur le cadavre ne peut donc pas toujours vous apprendre ce qui a eu lieu pendant la vie, et l'anatomie pathologique ne nous donne certainement le dernier mot ni de la nature des maladies, ni de leur siége, ni de leur traitement."—*Clinique Medicale*, vol. v. p. 179.

But while we assent to the truth of the observation conveyed in the above passage, we may with reason anticipate, arguing from what has already been accomplished, that in proportion as we continue to search for a connexion between the development of symptoms, and particular derangements of structure, in the same degree will our accuracy in diagnosis be increased. The communication, therefore, of cases where the symptoms have been accurately noted, and in which post-mortem examinations have been obtained, becomes valuable for the promotion of an object so much to be desired.

Influenced by such a consideration, I published in the seventh volume of this Journal the history of some cases of aneurism of the thoracic aorta, in which I had an opportunity of verifying the diagnosis by an inspection of the body, and endeavoured to draw attention to the possibility of detecting the disease when situated in particular portions of the vessel, before it unequivocally manifests itself under the form of a visible pulsating tumour. In the present paper I purpose to detail the history of another aneurism, situated, like the former, in the arch of the aorta, and which, by the aid of physical signs, in conjunction with general symptoms, was pronounced to exist some months before the tumour presented itself externally.

Previously, however, to the consideration of this case, I wish to advert to symptoms, which may be produced by any tumour situated in the cavity of the thorax; and which in many particulars resemble those arising from aneurism, and without close examination may be readily mistaken for them.

The tumours most commonly observed, are those which arise from the enlargement of the bronchial glands, owing either to chronic inflammation, or the deposition of tubercular matter in their substance. These glands are frequently found, as is well known, in the posterior mediastinum, presenting a hard uneven surface, and when laid open are found to contain a substance which has been compared to putty or mortar, sometimes intermixed with laminæ and spiculæ of bone; occasionally they consist of an abscess containing matter of a more fluid consistence, but still of a scrofulous character; and instances are alluded to by Andral, where the product of the inflammation was pure, healthy pus.

In the earlier periods of life, these different morbid accumulations are frequently evacuated through the trachea, or some of its divisions, and if tubercles have not been deposited in the parenchyma of the lung, (which unfortunately is the more frequent case,) the disease often terminates in this manner favourably.

In adults, and the middle aged, the bronchial glands are also found similarly affected; but the morbid collection is not so frequently ejected from the system in the manner just alluded to, indeed so much the contrary, that M. Andral has never seen an instance of it.*

The cases, however, I shall presently detail, prove that the trachea or bronchial tubes may be perforated in adults as well as in children by the ulcerative process produced by

* Clinique Medicale, vol. i. p. 182.

the proximity of the tubercular mass to these organs, and further, that symptoms of peculiar severity will arise, and other organic lesions ensue, which may lead to fatal results.

The first of these cases occurred in a woman æt. 30, and the second in a male idiot, aged 20. The most striking phenomenon in the former was an intense and obstinate dyspnœa; and in the latter, a general emphysema consequent on the rupture of an adhesion between an abscess in a scrofulous gland, and the right bronchus. No symptoms existed in either case which could be said to be characteristic of the disease, which corroborates the result at which Andral has arrived—who states, that whatever suspicions we may entertain with respect to the existence of enlarged bronchial glands, it is one of those affections which can only be discovered by a post mortem examination.

CASE I.—*Intense Dyspnœa and Bronchitis; an enlarged bronchial Gland, containing ossific and cretaceous Matter, communicating with the right Bronchus.*

Mary White, aged 30, was admitted into the Whitworth Hospital, House of Industry, on the 11th November, 1835. She had been complaining some time before admission of a slight cough, attended with difficulty of breathing; which symptoms she attributed to frequent exposure to cold and wet. Had never, up to the date of her present illness, been subject to any pulmonary affection.

November 12th.—When I first saw her this day, she was sitting up in bed, and breathing with great effort; her lips were of a livid colour; and her expression of countenance indicative of much suffering. She was seized at intervals with a cough of a spasmodic character, accompanied by a peculiar ringing sound. The sputum was frothy and not copious; her pulse small and quick, about 100; abdomen soft; bowels regular; catamenia scanty, but regular.

Percussion yielded a clear sound over the surface of both lungs, nor was there any difference in this respect at either

side. Extensive, sonorous, and sibilous rales were heard on applying the stethoscope to the antero and posterior surfaces of the chest; they were somewhat louder over the right lung; no difference in the respiration on either side was observable on applying the instrument to situations corresponding to the root of the lungs. She was immediately bled to the extent of twenty ounces, and ordered an ounce of the following mixture every third hour:

℞ Tart. Antim. et Potass. gr. vi.
Aquæ Cinnamoni ℥vi.
Guttæ Nigræ gts. vi.
Syrupi Aurantiæ ℥ii. M. Ft. mist.

13th. She found herself greatly relieved by the bleeding and tart. ant. mixture, and could lie down in the bed without producing a return of the dyspnœa. The bronchial rales were still loud; and the oppression of the chest though diminished, was considerable. The expectoration, which was catarrhal, was more free and copious, and did not contain any tubercular matter. Eight ounces of blood were taken by means of cupping between her shoulders; and the same medicines continued.

She continued to improve so much under this treatment, that I was in hopes the inflammation, which I considered to be an uncomplicated affection of the bronchial tubes, was subdued. Her respiration had become tranquil; the cough was much less severe, and had lost its laryngeal character. The tart. ant. was discontinued, and she was ordered the common cough mixture of the hospital: blisters of a moderate size were placed in succession over the sternum and between the shoulders.

26th. All the symptoms again returned with extreme violence; the dyspnœa in particular was excessive, for which she urgently demanded to be bled. She ascribed the recurrence of the attack, to an incautious exposure to cold the night preceding. Twelve ounces of blood were

taken from her arm, and the mixture of tart. ant. administered as before; a cathartic enema, with tincture of assafœtida, was likewise ordered, and a sinapism to be applied to the sternum. The phenomena observed on auscultation and percussion were the same as before. None of the physical signs of disease of the heart or great vessels could be detected, or of any other affection of the chest except bronchitis.

27th. All the urgent symptoms yielded a second time to the treatment above described, and the respiration became tranquil. I made another accurate examination of the chest, but did not observe any phenomenon indicative of compression on the trachea, or its primary divisions. The bronchial rales were still loud and extensive; a clear sound was obtained on percussion of both sides of the chest.

December 1st.—From this date she had daily returns of paroxysms of cough, accompanied by intense dyspnœa; venesection was again resorted to, which gave temporary relief, but evidently failed in preventing the recurrence of the attacks. The tartar emetic mixture also failed in making any decided impression on the disease. She was ordered, during the paroxysms, draughts composed of camphor mixture, tinct. of hyosciamus, and fœtid spirit of ammonia; leeches were applied to her chest, and sinapisms to the feet; I ordered her in the evening an emetic consisting of a scruple of hippo powder, and one grain of tartar emetic.

These measures gave her some relief for a few days; but on the 6th, the dyspnœa returned with extreme violence, accompanied with the same description of cough. Her ankles had now become œdematous, and the pulse small and feeble. Her chest was ordered to be dry cupped, and the medicines as last ordered to be continued. There was some remission of the dyspnœa in the evening; but in the night, Mr. Mayne, at that time clinical clerk of the Whit-

worth Hospital, was called to visit her. He found her labouring under such extreme difficulty of breathing, that as a last resource he again bled her; only four or five ounces of blood could be obtained, after which she breathed with more freedom. Early in the morning of the 7th, it appears from the nurse's account, she had another paroxysm, and in an attempt to sit up in the bed expired.

Post mortem Examination twenty-four Hours after Death. On removing the sternum, the pleura presented a natural appearance on both sides; no adhesions existed. The lungs were somewhat congested posteriorly; but no traces of pneumonia or tubercles were found. The trachea presented a natural appearance as far as its bifurcation, but from that point the mucous membrane of the bronchial tubes, in both lungs, was vascular and thickened, and the tubes were filled with a sanguinolent, frothy mucus. On running the finger along the interior of the right bronchial tube, the sharp edge of a plate of osseous matter was found impacted in its substance, which, when further traced, proved to be a portion of an earthy and osseous mass, contained in an enlarged bronchial gland. This gland was about the size of a large walnut, and of a stony hardness on its exterior; it was situated behind the bifurcation of the trachea, and had contracted adhesions to the right bronchus. Several other glands were enlarged, and contained cretaceous looking matter, but none of them had contracted adhesions to the trachea or its branches. A small quantity of serum was found in the lateral ventricles of the brain, the substance of which was sound: the veins of the brain and its membranes were congested. Nothing remarkable was found in the abdominal or pelvic viscera.

The most urgent symptom in this case was the dyspnœa, the cause of which was only discovered on the post mortem examination. Not being able to trace any symptoms of an affection of the larynx, or of the heart, my attention was

directed to the state of the respiration at the root of the lungs, as I had observed, not long before, a cough of a similar description, accompanied with severe dyspnœa, arise from the pressure of an aneurismal sac on one of the divisions of the trachea. The non-existence, however, of an abnormal pulsation in the chest, together with the absence of all the other physical, and most of the rational signs of that disease, convinced me that the recurrence of the symptoms was attributable to some other source of irritation. There was no indication, however, during life, which could lead me to infer the existence of the affection from which it was afterwards ascertained all the symptoms originated. I closely examined the sputum, but did not observe any caseous matter, or other morbid product, which is sometimes, as I have already observed, evacuated through the air passages from diseased bronchial glands; neither was there any appearance of disease in the cervical glands; nor did the patient present the usual appearances of a strumous diathesis.

I have stated that there was no difference in the force of the respiratory murmur in either lung, during the intervals of the spasmodic attacks.

This is accounted for by the nature of the connexion between the gland and the trachea: the tumour in fact did not compress the trachea; and although it had contracted adhesions to the right bronchus, the calibre of the latter was not thereby in the least altered.

Whenever the respiratory murmur is permanently feeble throughout the lungs, and provided we are able, by percussion and auscultation, to satisfy ourselves that it is not owing to disease of the organ itself, or its membranes, or of the heart, we must seek for the cause of the phenomenon in some mechanical obstruction existing at the root of the bronchial tubes. This obstruction may arise from the existence of various tumours in the anterior or poste-

rior mediastinum, either an enlarged bronchial gland, a malignant or an aneurismal tumour. There are several signs accompanying the latter, which in many cases may enable us to recognize it; but I am not aware of any which can with *certainty* lead us to a knowledge of the former.

Andral expresses himself very decidedly on this point, and observes, that although in some cases we may suspect, we cannot be certain of the existence of enlarged bronchial glands. He gives an interesting case in his second vol. of the Clinique Medicale, which, as it bears upon the subject, I shall here introduce.

"A man, aged 38, had been in good health until the month of February, 1824, when he was attacked with a cough which continued to the month of May, when he entered the hospital of La Charité. His voice was hoarse, and his respiration short; he had never spit blood.

"When he presented himself for examination, his chest was found to be equally sonorous throughout; the respiratory murmur was strong and clear through the whole of the right side; *in the left, on the contrary, it was remarkably feeble, but otherwise natural.* The only symptom the patient complained of was a difficulty of breathing, which was sensibly increased not only by walking on an inclined or horizontal plane, but even by a quick movement in bed. The dyspnœa also increased after meals. He could lie on either side equally well. His cough was not frequent, and the expectoration was catarrhal. There was no symptom of disease of the heart or great vessels. Slight accessions of fever took place from time to time; the face was pale, and nutrition was sensibly altered. Soon after the patient spit blood at different times: the habitual dyspnœa became gradually more aggravated.

"What was the cause of this great difficulty of breathing and the other morbid phenomena? The continuance of the cough; the recent hæmoptosis; the accession of fever from

time to time; and finally the emaciation, were strong reasons for inducing us to fear the existence of tubercles disseminated through the parenchyma of the lungs: but the sense of suffocation was much more considerable than that usually produced by tubercles; and above all, we could hardly explain by them the greater feebleness of the respiratory murmur on the left side than on the right: for unless miliary tubercles are sufficiently numerous to constitute, in some degree, a mass by their conglomeration, or unless the pulmonary parenchyma between them is hepatized, (and then the sound would be dull,) the respiratory murmur, far from being weak, is augmented in intensity. Proceeding then by way of exclusion, we were led to think that the principal bronchus of the left side was compressed by a tumour, owing perhaps to a tubercular mass of bronchial glands, thence the introduction of a smaller quantity of air into the left lung, and consequently the dyspnœa. The patient left the hospital without any visible change in his condition."

The dyspnœa here was the principal symptom, which M. Andral ascribes to the compression of the left bronchial tube; but in the case I have detailed, no such compression existed, as was evident both by the force of the respiratory murmur being equal in either lung, and from observing the unaltered condition of the trachea and bronchial tubes after death.

This symptom, therefore, would seem to have originated from the irritation of the phrenic nerves, which were in close contact with the tumour. M. Berrard has communicated a similar case,* where no other lesion was found to explain the excessive dyspnœa from which the patient sufferred during life, but a tumour developed in the substance of one of the phrenic nerves, both of which were altered in

* Cliniq. Medical. vol. i. p. 262.

colour and consistence. In the case of White they were not so altered, but must have suffered either compression or irritation from the manner in which they were circumstanced with respect to the tumour. Both these instances prove equally well as experiments instituted for the purpose, how much the integrity of the phrenic nerves is necessary for the due performance of respiration. When the pneumo-gastric are implicated in these tumours, the dyspnœa arises from another cause, the imperfect oxygenation of the blood; which process, as experiment has proved, is immediately influenced by any lesion affecting these nerves.*

It is not, however, always possible to connect the difficulty of breathing in these cases, with any one of the three causes above mentioned. The trachea and bronchial tubes may be of their natural calibre, and the phrenic and pneumogastric nerves either unconnected with the tumour, or unaltered in structure. A remarkable case of this kind occurred to me lately, where an abscess in an enlarged bronchial gland communicated both with the œsophagus and a bronchial tube. The symptoms were those of acute bronchitis, accompanied by violent dyspnœa; and eventually an emphysematous distention appeared about the neck, which gradually extended over the body. The following are the particulars, as far as I have been able to collect them.

Case II.—*Double Bronchitis with intense Dyspnœa; Emphysema of the cellular Tissue of both Sides of the neck and left Side of the Trunk; an Abscess in an enlarged bronchial Gland, communicating with the left bronchial Tube.*

This case occurred in a male idiot, æt. 20. He was sent to the Hardwicke Fever Hospital from one of the

* Vide J. L. Brachet, Recherches expérimentales sur les Fonctions du Système Nerveux Ganglionaire, from p. 137 to 174. I may further remark, that in the case of this woman, the dyspnœa could not be traced to any derangement of the

lunatic wards in the House of Industry, on the 23d August, 1836. I saw him on his admission, but found great difficulty in obtaining any information with respect to his symptoms, as he could not return a rational answer to the questions put to him. His right fore-arm was flexed upon the arm, the fingers were permanently contracted on the palm of the hand, and the whole extremity was wasted. I understood from the nurse that he had symptoms of fever, with an occasional dry cough, for two or three days previous to his admission. On applying a stethoscope, I found the respiratory murmur equally audible in both lungs, accompanied by bronchial rales. There was dulness on percussion under the clavicles. He had a quick compressible pulse; his skin was hot and dry; and his bowels had not been moved for two days. He was ordered purgative pills composed of calomel, cathartic extract, and James's powder, with a draught of castor oil, and tincture of senna, in the evening.

Unavoidable absence from Dublin prevented me seeing this case for a few days. Mr. Russell, the medical clinical clerk, has, however, furnished me with the following account of the progress of the symptoms. On the 25th he was observed to cough more frequently, and his respiration became hurried and difficult: extensive bronchial rales were heard in both lungs. The expectoration was catarrhal and scanty: he lay on his right side. Bleeding and other measures were taken to arrest these symptoms, but they continued to increase in severity. On the 26th the cough came on in frequent and violent poroxysms; the breathing became more difficult and wheezing; and the lips and extremities assumed a purple tinge. The expectoration was

uterine function, which practitioners are well aware will occasionally produce this symptom to an alarming extent. Neither did it depend on the evolution of flatus in the stomach and intestines, consequent on the reception of indigestible food, from which cause a very violent dyspnœa will sometimes suddenly arise.

more copious and purulent looking; he still continued to lie on his right side.

On the 27th I again saw him, and was surprised to find an emphysematous distention of the integuments of the left side of the neck and thorax. The loud crepitus produced by the emphysema prevented me from ascertaining the state of the respiration in the left lung, but muco-crepitating and sonorous rales were heard throughout the right. No results could be obtained on percussion, by reason of the emphysema of the integuments. The difficulty of breathing had now become excessive; the extremities cold and blue, and the pulse at the wrist feeble and intermitting. Mr. Hutton, at my request, examined the chest, but could not discover any injury which might have given rise to the emphysema. I, therefore, supposed an air cell had been ruptured in one of the paroxysms of coughing, and that the air had been extravasated into the cellular tissue about the roots of the great vessels.

The difficulty of breathing did not appear to depend altogether on the emphysema, as it existed some time prior to the appearance of the latter. I attributed a good deal of it to the want of power of expectorating the matter, which, as appeared from auscultation, was lodged in considerable quantity in the bronchial tubes. It was difficult to pursue any plan of treatment with this idiot, as it was sometimes impossible to persuade him to take the medicines which were ordered. To promote the expectoration, he was ordered camphor mixture and decoct. of polygula, with carb. of ammonia. A blister was applied between his shoulders, and sinapisms to his feet. Although the effusion of air was considerable about the neck and chest, it did not appear to extend so fast as to lead to the apprehension of sudden death. In the evening, however, he was seized with repeated paroxysms of coughing, during which the emphysema rapidly extended: he continued to suffer from cough

and dyspnœa during the night, and expired on the morning of the 28th.

The examination of the body was conducted by Mr. Hutton, when the following appearances were observed :—

The integuments of both sides of the neck and face were distended with air as far as the temples. The emphysema was much more considerable on the left side of the trunk than the right. The subcutaneous cellular tissue of the extremities was not much distended; that of the palms of the hands and soles of the feet not the least so. The submucous cellular tissue of the eyes and nose was not injected, neither the subserous cellular tissue of the abdomen. It was observed that the left tunica vaginalis testis contained a considerable quantity of air, which quickly escaped on a puncture being made; after which the scrotum returned to its natural dimensions. This circumstance is very remarkable, and difficult to account for. No air was found in the abdominal cavity. On opening the chest, the cellular membrane in the anterior mediastinum was found completely filled with air, which extended as far as the root of the left lung, but did not permeate the cellular tissue of the organ itself: no air whatever was found in the pleural cavities. Isolated tubercles in their primary stage were found in both lungs. On passing a probe down the left bronchial tube, a small opening was detected, which led to an abscess in an enlarged bronchial gland, containing scrofulous matter; it had contracted adhesions to the posterior border of the left lung, and to its corresponding bronchial tube. This latter adhesion had been ruptured, and through this rupture the air was extravasated into the cellular membrane of the posterior mediastinum. The abscess also communicated with the œsophagus by another opening in its posterior wall.

The brain of this idiot was very small: a serous sac was discovered on the superior surface of the left hemisphere.

The connexion of this lesion with the state of the right upper extremity is remarkable; but as it is not connected with the subject of which I am treating, I shall not here enlarge upon it. Mr. Hutton made a particular examination of the brain, and will probably communicate the particulars on a future occasion.

The great difficulty of breathing was here owing to two causes. First, the extravasation of air into the cellular membrane of the mediastinum, by which the free expansion of the lungs was prevented. And, secondly, the function of these organs was already impeded by the deposition of tubercles in their parenchyma. The impediment to the respiration thus produced rapidly increased, and proved fatal much sooner than I expected, and before the general cellular texture of the integuments or of the lungs was injected with air. The free communication existing between the cellular membrane of the mediastinum and of the neck favoured the extravasation in this direction, and will account for the absence of it in the cellular texture of the lungs, where the tissue was more condensed from the deposition of the tubercles.

The production of emphysema in the manner above related is very unusual: I am not acquainted with an instance precisely similar. Dr. Townsend* says, "We are not acquainted with the records of any case in which perforating ulcers of the larynx or trachea have led to the formation of emphysema, a fact which is probably to be accounted for by the air being prevented from entering the cellular tissue by the adhesive inflammation which usually precedes and limits the ulcerative process." In the greater number of these cases, the adhesive inflammation will, no doubt, prevent the effusion of air through the trachea into the cellular texture; but while the adhesions are yet recent,

* Encyclop. of Practical Medicine, Art. Emphysema.

as was the case in the instance last mentioned, they may be ruptured by some sudden respiratory effort, and thus give rise to the phenomenon.

The two cases I have adduced prove, that, in the adult as well as in the child, the morbid matter in these glands may be ejected from the system through the air passages; and that during the stages of this process, unforeseen and fatal complications will occasionally arise. This circumstance renders it desirable to promote, if possible, the absorption of the morbid deposit, and thus prevent the occurrence of these casualties. The difficulty, however, of ascertaining the existence of these tumours, and the frequency of such symptoms, as spasmodic cough, dyspnœa, &c. from other causes, often prevent our employment of the remedies so efficaciously directed to the discussion of scrofulous tumours externally situated. The physical signs, of such use in diagnosis in many diseases of the chest, are, in a great measure, here absent; or, if any exist, the difficulty of assigning them to their proper cause seems to me to be very great. In the case I have quoted from Andral, there was, in all probability, a permanent compression of the left bronchial tube, and so far there was a physical indication of a tumour situated on its exterior. In confirmation of this opinion, a case may be referred to, (Oberr. 4, vol. i. Cliniq. Medical. p. 190,) presenting similar physical signs, which were afterwards ascertained to be owing to the pressure of a tumour on the right bronchus. But that such evidence does not always exist, is clear from the cases I have detailed, and from observing after death the unaltered calibre of the trachea and its divisions, in many similar cases.

It will be seen from the cases which I have detailed that, when the enlarged glands are situated about the bifurcation of the trachea, the following symptoms have ensued, viz., hoarse voice, wheezing respiration, cough sometimes of a violent spasmodic character, accompanied with a ringing

sound and dyspnœa or orthopnœa. The physical signs may consist of feeble respiration in one lung, coinciding with a clear sound on percussion. The above enumeration is not intended as a complete one of all the symptoms which are present in any individual case: for, on the one hand, either from the small size of the tumour, or from its situation in the posterior mediastinum, most, if not all of them, may be absent; and, on the other, from the increased size of the tumour, or from its position, additional symptoms may supervene. Thus, from the latter causes, all the phenomena arising from impeded circulation in the upper extremities, from pressure on the œsophagus, or even from displacement or irregular action of the heart,* may be present; while again, instances daily occur, where, in our examinations, we find these tumours in such situations, that they could not have produced the above symptoms during life.

The practical point to consider however, is, if all or a great number of these symptoms are present, whether it be possible to infer from them the existence of a tumour in the cavity of the thorax, and if so, what may be its nature. It is evident that, before we can arrive at such an inference, we must satisfy ourselves of the non-existence of organic disease of the heart, lungs, great vessels, and other organs situated in the thoracic cavity; and also take into consideration the various causes on which the individual symptoms may depend. A single symptom is, comparatively speaking, of little value, if considered by itself; but may

* Mr. Adams has informed me, that some years back he was requested to examine the body of a person supposed to have died of disease of the heart. On examination, this organ was found unaltered in its structure, and of its natural dimensions; immediately behind it an enlarged bronchial gland, commencing at the bifurcation of the trachea, was discovered, the irritation of which might have produced the irregular action of the heart, which was supposed to depend on disease of the organ.

become highly so, if taken with many others, all leading to the same conclusion.

Thus, supposing we meet with a case where the symptoms are such as are above detailed, and that by careful examination we are able to exclude all causes of the phenomena, except those that depend on direct pressure of the air tubes, the great vessels, œsophagus, &c., the next question for us to determine will be, what is the nature of the compressing body? Here, however, we find considerable difficulty, inasmuch as any tumour, whatever be its structure, will, if particularly circumstanced, produce many, if not all, of the general and physical signs just alluded to. Under these circumstances we must seek for additional information in the history of the case, the appearance of the patient, and the nature of his constitution. In children, for instance, the presumption is in favour of a scrofulous tumour, as is proved by the results of Louis and Andral's investigations with respect to the comparative frequency of the deposition of scrofulous matter in the bronchial glands at this age. This inference will be strengthened, if the cervical or mesenteric glands are enlarged. In the adult, also, a similar presumption may be formed, if either scrofulous tumours exist in the neck, or if the usual signs of a strumous diathesis are present. Should we be deprived of the assistance to our diagnosis, derived from such considerations, feeble as it must be admitted to be, we are nearly left to conjecture as to the nature of the tumour, provided we cannot trace the symptoms to the development of an aneurism. Thus osteo-sarcoma, fungous, or encephaloid tumours may exist; and I am not aware of any symptoms, general or local, which will enable us with certainty to say that the physical impediments to the functions of deglutition, respiration, or circulation, arise from their existence in the chest. The only circumstance which might, in such a case, guide us

would be the appearance of a malignant tumour of known constitutional origin, visible in some part of the external surface of the body, or from ascertaining the development of tumours in the abdomen, or along the neck, &c. That these considerations are not without weight, particularly if we are satisfied of the absence of disease of the heart and lungs, will sufficiently appear from the history of some malignant tumours situated in the thorax, which is given by Dr. Sims.* In two of these cases malignant tumours were found engaging the heart and lungs, and were conjectured to be the cause of a peculiar train of symptoms during life, from the impossibility of accounting for them by disease of the thoracic viscera, or as Dr. Sims expresses it, by "trying the diseased state of the thorax by seclusion."

In many other instances, however, these tumours are only discovered after death: such was the case in an individual admitted into the hospital of La Charite. Andral states† that a large mass of melanosis was found occupying the bifurcation of the trachea. The symptoms were cough, with purulent expectoration and feeble respiration in the right lung, the cause of which was not suspected during life.

The case, however, is different in aneurisms of the great vessels in the thorax; in some of these the disease, originating from a vessel near the superior outlet of the chest, quickly makes its appearance in the form of a visible pulsating tumour; the nature of the affection is then in most cases readily ascertained, and often gives opportunity for that bold application of the knife and ligature, which constitutes one of the most brilliant results of modern surgery

But in other, and I believe in the majority of cases, the. aneurism is deeper seated, and unless devoloped in situations

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* Medico-Chirurgical Transactions, vol. xviii. p. 282.

† Clinique Medicale, vol. i. p. 190.

where it must manifestly interfere with important functions will often continue to increase in size, without awakening even a suspicion of its existence. Those situations I have in a former paper alluded to as being principally in the descending portion of the thoracic aorta, and in other portions therein mentioned. No case has since occurred to me, from which I could study any peculiarity which aneurisms in these situations may present; but I have had an opportunity of observing another instance of the disease in the arch, where, as the event proved, the rational, combined with the physical signs, were found sufficient to diagnose the affection before it manifested itself under the form of an external tumour. The feature which first arrests attention in an aneurismal tumour, and which will best distinguish it from all those to which I have adverted, is pulsation. If we can clearly establish the fact that a pulsation exists in the chest apart from that of the heart, it is undoubtedly taken as a single sign the most to be relied on of any to which this disease gives rise.* The absence of pulsation, however, does not always argue the absence of an aneurism. Many circumstances connected with the situation of the tumour will prevent us from perceiving the impulse, or it may be so feeble from the recent development of the sac as to render it inappreciable to the ear or stethoscope. But whenever it is so distinct as to give a perceptible shock to the instrument, it should excite our attention, and lead us to examine, as far as we can accom-

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* The fallacy of trusting to a single sign on which to found a diagnosis is strikingly illustrated in a case detailed by Dr. Gordon, Medico-Chirurgical Translations, vol. xiii. p. 12. A small round tumour presented itself in a young woman below the sternal extremity of the left clavicle; it pulsated regularly and strongly, and was pronounced at consultation to be an aneurism of the aorta or arteria innominata. It continued to increase, and she died of fever and exhaustion. On examination, a tumour was found in the anterior mediastinum, containing hair, fatty matter, bone, and teeth.

plish it, the condition of the organs in its immediate vicinity. Thus if a case presents itself wherein this impulse is perceived in a situation, suppose, corresponding to the root of the lungs, we are next to inquire into the state of the respiration, and if an impediment appears to exist to the free ingress of air into these organs we connect these two phenomena together in the relation of cause and effect, and ascribe the feeble respiration to the mechanical pressure.

In like manner the co-existence of dysphagia, when it exists, and is referred by the patient to a situation corresponding to the impulse, becomes a valuable indication, and the same may be said with respect to the pulse of the radial artery being feebler on the side of the impulse than on the opposite, and also of the œdema, numbness, &c. It is thus by tracing back the phenomena to their dependence on a mechanical obstacle, and observing that where this obstacle appears to exist a pulsation is manifest, we approach step by step to a solution of the difficulties which many of these cases present. There is another circumstance also which I may here mention connected with the *history* of the case, which it is important to remember. In many instances patients refer the origin of their disease to the receipt of an injury; either a blow received on the front of the chest or between the shoulders. In three cases out of four which have come under my own observation during these last three years an injury of this description had been received, and the origin of the complaint dated from thence.

I cannot undertake to say that inflammation of the lining membrane of the aorta is a necessary or a common consequence of such an injury, but there is good reason to suppose that those degenerations of structure in the arterial tissues which involve a loss in their elasticity, and which are so frequently observed to accompany aneurismal tu-

mours, are the result of previous inflammation. Bouillaud* has entered largely into this subject in his late work on diseases of the heart, and has laboured with much success to shew that disease of the valves of this organ is the sequela of inflammation of its lining membrane.

If this supposition be correct with respect to the morbid conditions of the internal membranes of the heart, it will apply with equal force to that of the aorta; and although the inflammation in the former case is generally idiopathic, instances are not wanting where it can be traced to direct injury. Without, however, entering further into speculative opinions on this subject, I can assert that in several cases, though not in all, both of disease of the heart and the aorta, the patients have alluded to severe injuries received some time before the commencement of the symptoms.

With the exception then of some cases, where from position, and other circumstances formerly adverted to, the symptoms are either obscure or equivocal, there are, as it appears to me, grounds for distinguishing aneurismal from other tumours situated in the thoracic cavity. In addition to the cases I have already published, illustrating the value of physical signs in conjunction with rational symptoms, as they have been termed, for distinguishing this disease, I shall here insert another in which I had an opportunity of verifying the diagnosis by a post mortem examination.

CASE III.—*Aneurism of the Transverse Portion of the Thoracic Aorta; sudden Death from Extravasation of the Contents into the left pleural Cavity.*

Peter Blake, æt. 30, a tall muscular man, had enjoyed good health until about four years back. He had been a private in a cavalry regiment for ten years, during which

* Maladies du Cœur, Art. "Endocardité."

time he was in the habit of drinking freely. About four years ago, while preparing his horse for duty, he received a kick from the animal in the back part of his chest, one hoof striking him between the inferior angle of the scapular, and the spine of the left side, and the other taking effect on the arm a little above the elbow; he was knocked down by the blow and violently stunned.

He felt great pain, increased by any movement of the upper extremities in the part of the chest injured, for nearly a year after the receipt of the accident: it then disappeared, but returned with increased severity in two years afterwards, when it was found necessary to discharge him from his regiment. He stated he was not bled or leeched, but was ordered purgative medicine; and, afterwards, mercurial ointment to be rubbed into the spine, with the occasional application of sinapisms to the back.

On his admission into the Whitworth Chronic Hospital, House of Industry, January 5th, 1836, he did not present any appearance of ill health; his tongue was moist and clean; appetite good; and his skin moist and cool. He complained of palpitations of his heart on the least excitement; also, of pain in the chest about the upper third of the sternum, and occasionally shooting backwards to the spine: this pain was relieved by pressure, but increased by a deep inspiration. He did not complain of the slightest pain when the spine was forcibly pressed with the hand, or rotated on its axis; nor had he cramps, œdema, numbness, or decrease of temperature, in either of the upper extremities. The right jugular vein was turgid; the pulse in both wrists the same in character and frequency,—72. He had a dry cough, unaccompanied by any peculiarity of sound. His voice was natural. He complained of difficulty in swallowing, and referred to the sterno-clavicular articulation, on the right side, as the seat of the obstruction. His chest was well formed; and no tumour visible in any

portion of it. His bowels for several days together were obstinately constipated.

The whole chest, except ever the upper third of the sternum, and to a short distance at either side of it, in the subclavicular regions, yielded a clear sound on percussion. The respiratory murmur was very audible throughout the left lung, except to a small extent under the clavicle, where it was feeble. The murmur was much feebler under the right clavicle and in the axilla; an impediment appeared to exist to the entrance of air through the right bronchus, which was overcome by a deep inspiration. No crepitus; but some sonorous rales were heard in both lungs. The sounds and impulse of the heart were natural. On applying a stethoscope, an impulse was distinguished along the course of the aorta, increasing in intensity, and greatest about the junction of the first and second pieces of the sternum.

On making him walk briskly for some time, the impulse here, both to the ear and to the stethoscope, was manifestly greater than that of the heart. A very obscure impulse was heard between the scapula and the spine, beside the bodies of the third and fourth dorsal vertebræ, on the left side. Two sounds were heard in the situation of these impulses, but unaccompanied by any bruit de rapé or soufflet. No fremissement could be distinguished, or any pulsation, on pressing the fingers behind the clavicles.

From comparing the physical signs furnished by the stethoscope, with the other symptoms, such as difficulty of swallowing; wandering pains through the chest, &c.; together with the fact of his having received a severe injury in the course of the descending aorta; I felt little hesitation in referring the whole of the phenomena to the development of an aneurism in some portion of this artery.

The obstinate nature, and peculiar character of the pains which he complained of in his chest, were further confir-

matory of this view. He had been under treatment in several hospitals without receiving any permanent benefit; and was discharged from the army, as he stated, for confirmed rheumatism and spasms of the diaphragm.

The treatment adopted was the occasional detraction of blood from the arm to the amount of six or eight ounces; the application of leeches to the seat of the pulsation; and the administration of ten drops of tinct. digitalis three times a day; his bowels were freely opened by purgative medicines; and he was enjoined perfect rest in the horizontal position. A moderate quantity of meat was allowed him daily.

He continued on this treatment for upwards of six weeks; and experienced considerable relief from the pains in his chest, which were so far relieved as to enable him to enjoy several hours of refreshing sleep; his appetite remained unimpaired; and his general appearance unaltered.

During this time his chest was frequently examined. He was seen by Dr. Crampton and Mr. Carmichael, and also by Mr. O'Beirne, Hutton, Adams, and M'Donnell, Surgeons to the Richmond Hospital, and they fully agreed with me as to the nature of the affection. As no material difference occurred in the symptoms, I only entered in my case-book the result of an observation made about seven weeks after his admission into the hospital. It is as follows:

February 23rd. Considerable dulness on percussion under the superior third of the sternum; dulness of the præcordial region not greater than natural; respiratory murmur feeble along the whole of right lung posteriorly; also, feebler in the upper lobe of the left lung. About the junction of the cartilage of the third rib with the sternum, on the right side, inspiration appeared to be arrested by some obstacle; this occurred about an inch below the place where he referred the dysphagia. The

upper third of the sternum was now visibly elevated by an impulse from beneath; on applying the stethoscope to this situation, a forcible impulse was communicated to the ear, and two sounds, accompanied by a slight bruit de soufflet, were heard in this direction. The upper extremities were colder than before; and great turgescence of both jugular veins was visible on the least exertion: he again complained of severe stinging pains along the inferior margins of the ribs, and across the antero-posterior diameter of chest. In addition to the treatment already adopted, a blister was applied to the upper third of sternum, and the surface of it afterward sprinkled with four grains of powdered digitalis.

The symptoms of an aneurism of the thoracic aorta were now, as I considered, very evident, and may be thus briefly enumerated. After a brisk walk, a visible pulsation was observed under the top of the sternum, distinct from, and more forcible than that of the heart. A strong impulse communicated to the stethoscope in this situation, diminishing in intensity towards the heart, but audible under the right clavicle, and more obscurely in the interscapular regions. Inspiration impeded at the root of the right lung, apparently from mechanical compression,—a consequent feebleness of the respiratory murmur in that lung: a similar feebleness, but to a much less degree, in the upper lobe of the left lung, (probably from the existence of a mechanical impediment to the expansion of its tissue, as the murmur was very audible throughout its other lobe, and the above phenomena could not be referred to disease of the organ or its membranes). Dulness on percussing the upper third of the sternum, and extending gradually under both clavicles. The descent of the morsel arrested in a situation nearly corresponding to the impediment to the ingress of air through the right bronchus.

This patient now became anxious to visit his friends

living at some distance from Dublin; I remonstrated with him on the impropriety of the step he was about to take, and pointed in forcible terms to the danger of a rapid aggravation of his sufferings if he did not submit to medical treatment. He, however, persevered in his determination, but promised if he felt worse to return to the hospital.

In about a fortnight he sent me word that he was much worse after the journey to the country, and requested to be again admitted under my care. On his admission next day a remarkable change had taken place in his figure, and the expression of his countenance. I found him propped up in the bed, supported by pillows, having found himself unable for the last three nights to lie on either side, or on his back. There was a great inclination of the trunk to the let side: he was thinner, and appeared to have undergone much suffering. He stated that he had suffered excruciating pain in the back from the motion of the vehicle on the road; and felt the beating pain in his chest more violent than ever. I suspected from this account, and from his general appearance, that the aneurism had made rapid progress, and that the bodies of the dorsal vertebræ had given way. This suspicion was confirmed on examining the back of his chest; a large pulsating tumour, commencing at the inferior angle of the left scapula, extending upwards under that bone, and across to the spine, was immediately perceived. The whole of the back, on this side, appeared to be heaved out, as it were, by the impulse communicated from the interior of the chest. The spinal column was distorted, and the spinous processes of the lumbar vertebræ seemed to stand prominently forward; those of the dorsal, opposite to the tumour, being inclined to the right side. The left hypochondrium was full and prominent. Considerable changes, as might be expected, had taken place in the physical signs. The following were the principal, as taken from my case-book:—

March 8th.—Great dulness on percussion over the superior half of the sternum; as also, over nearly whole of the *left* lung. The impulse at the sternum not so sensible to the hand as formerly; but forcibly so in the situation of the tumour behind. Two sounds, unaccompanied by any or very slight bruit de soufflet, in these situations. The reverberation of the voice lost on the left side. Respiration audible in the left axilla, but suddenly ceasing below a horizontal line corresponding to the left mamma; also audible, but of a bronchial character, in the right axilla. Pain in the tumour, of a burning, lancinating character, remittent, and relieved by pressure. Pulse 100; weak, but regular; loss of appetite, and sleep; obstinate constipation; œdema of eyelids and ankles; loud dry cough.

It was evident from this examination, that the aneurism had greatly extended since he left the hospital; and that the fatal result was not far distant. He was bled to the extent of eight ounces; and twelve leeches were applied to the dorsal tumour. Pills composed of calomel, cathartic extract, and croton oil, were ordered to be taken immediately, and a cathartic enema in the evening. The next day I found him somewhat relieved from the pains in his chest; his bowels had been opened freely; and he had slept for three or four hours. He was ordered half an ounce of the infusion of digitalis, three times a day; and an anodyne of fifteen drops of acet. tinct. of opium at night. He appeared to suffer less uneasiness on the next and following day. On the evening of the latter, March 11th, he attempted to sit up in bed to procure a drink of whey; in making the effort, he uttered a loud shriek, and fell back in the bed; the patient nearest to him immediately ran to his assistance, but he refused to be raised, and stated that he was going to faint. His lower extremeties immediately became convulsed; and in a few minutes after he expired.

Post Mortem Examination eighteen Hours after Death.—The lips, extremities, and entire surface of the body appeared pale and exsanguineous. No trace of blood was visible in the mouth or nares. Left side of the chest very dull on percussion. On removing the sternum, the heart was found considerably to the right of its normal position. The pericardium had contracted adhesions to the left lung. No adhesions of the pleura to each other were found anteriorly. The lungs were pale and apparently healthy, except on their edges, which were slightly emphysematous. On drawing out the left lung, a dense coagulum of blood, five pounds in weight, was discovered extravasated into the pleural cavity; when this coagulum was removed, the pleura costalis was found to be elevated from off the ribs by blood effused underneath. Opposite the angle of the sixth rib, a lacerated opening existed in this membrane, corresponding with one about the diameter of a quill, in a large aneurismal sac; through this aperture the extravasation had taken place. The aneurismal tumour was seven inches in length, and six and a half in a transverse direction. Its antero-posterior diameter was four and a half inches. The opening communicating with the aorta was situated on the posterior wall of the latter, about half an inch from the origin of the left subclavian artery; its shape was eliptical; longest diameter nearly one inch. The bodies of the dorsal vertebræ, from the second to the ninth inclusive, were more or less removed by absorption; as also, part of the fifth and sixth ribs on the left side, between their heads and angles. The sac, which was lined with a dense coagulum, lay in front and at either side of the spine, but extending much further to the left side than to the right. The trachea descended along the front of the tumour; the right bronchial tube was remarkably and permanently flattened; the left slightly so. The œsaphagus was adherent to the right of the whole length of the sac.

the adhesion commenced opposite the sterno-clavicular articulation. Steatomatous matter, more abundant near the mouth of the sac, was observed under the lining membrane of the aorta. The substance of the heart and its valves presented no trace of disease; left ventricle was found strongly contracted.

Considering the magnitude of this aneurism, and the length of time the patient had been complaining, it must be evident that the disease had existed for a long period. When, however, he first presented himself for examination, no tumour was visible, and the symptoms, if alone considered, might be referred, without closer examination, to disease of the spine.

The difficulty of deglution, which was ascertained by making the patient swallow a mouthful of bread, first excited suspicion that an aneurism existed; and this was confirmed by an examination with the stethoscope.

It will be observed from the history of the case, that the signs furnished by the instrument established the fact, that the right bronchial tube was compressed, and that a portion of the left lung was not freely expanded in the process of inspiration; phenomena which were inexplicable on the supposition that the spine was alone affected. It was further proved, that the heart and lungs were free from organic disease, and hence that the cause of the feeble respiration in the right lung, and in a small portion of the left, was to be ascribed to the development of a tumour in the vicinity of the parts whose functions were thus impeded.* The stoppage of the morsel was confirmatory of the same view. The nature of the tumour next became a sub-

* Feeble respiration may be present in one lung or some portion of it, from *contraction* of a bronchial tube. Vide Andral's Clin. Med. vol. i. Obser. 2 p. 184. But in Blake's case the dysphagia could not be accounted for on this supposition, hence the necessity of taking all the symptoms into consideration.

ject for consideration, and here not only the pulsation, but the peculiar expansive character of it, led to the inference that it was an aneurism. Such were the grounds for the diagnosis given on the first examination, and it will be seen from the post mortem inspection, that all the phenomena could be fully accounted for. The right bronchial tube was completely flattened by the pressure of the sac behind it, and the expansion of the latter to the left gradually exercised a compression on the upper lobe of the left lung. The œsophagus had contracted adhesions to the right side of the sac, and it was to the right of the abnormal pulsation the dysphagia was referred.

As the disease advanced, additional symptoms became manifest. Percussion elicited a dull sound over a considerable space corresponding to the upper portion, and on either side of the sternum; as also over the whole posterior surface of the chest on the left side. The resonance of the voice, and the respiratory murmur gradually became extinguished in these situations. The right side, with the exception of the regions mentioned, alone continued to yield a clear sound on percussion; while the free expansion of the lung could not be accomplished by the diminished volume of air admitted through the compressed bronchial tube; and hence the feeble respiratory murmur of this side. The situation of the sac explains these phenomena. The greater portion of its fundus lay in the left side; thus preventing the play of the lung, and producing the dull sound on percussion; while its right extremity pressed more directly on the corresponding bronchial tube, and less on the right than the left lung.

Finally, the pulsation became manifest to the eye; the top of the sternum was protruded forwards, and a powerful shock was communicated to the hand and instrument when applied to this situation. The respiration became more hurried, the loud dry cough and burning pains around and

in the chest more incessant; and at length the tumour forced its way through the osseous parietes of the chest, at the subscapular region of the left side.

Notwithstanding the great absorption of the bodies of the vertebræ, it is singular that no pain was complained of by the patient even within a month of his death, when the spine was struck or rotated on its axis. The simple removal of the bone seemed to be effected by the pressure of the sac, without producing any inflammation more extensive than what was evidenced by its adhesions to the structures in its vicinity. The fact is important, as shewing how little reliance should be placed on the absence of pain in this situation, as affording a contra-indication to the existence of the disease. The same remark is applicable to the freedom from cramps of the upper extremities and of numbness and œdema; all of which may or may not be absent, according to the position of the tumour with respect to the great vessels and nerves. The dyspnœa in this case was not so urgent as in the two first cases of scrofulous tumours, where the bronchial tubes were not in the least compressed.

The result then of the foregoing cases and of others which I formerly published, appears to warrant the following conclusions with respect to tumours developed in the thoracic cavity.

1st. That no single physical sign, taken by itself, can characterize the nature of any particular tumour.

2nd. That in many cases the presence of an abnormal pulsation in the chest, when detected by the stethoscope, affords the best indication of the existence of an aneurism, and may frequently serve to distinguish it from other tumours attended with nearly similar physical signs.

3rd. That the same rational symptoms, as they have been termed, may be produced by any species of tumour within the cavity.

4th. That the morbid contents of enlarged scrofulous glands are sometimes evacuated through the air passages in adults, as well as in the earlier periods of life; and that during the process, death may occur from excessive dyspnœa and bronchitis, or from general emphysema.

The analysis of the physical signs and general symptoms, observed in the above case of aneurism, and of three others which I formerly published, is as follows. In all these four cases the disease was situated in some portion of the arch of the aorta.

First of the Physical Signs.—A single or double impulse in situations more or less distant from the heart, was discovered by auscultation in the four cases.

Two sounds, more or less distinct, accompanied this impulse. A bruit de soufflet was heard in two, and a bruit de rape in one case.

A feebleness of the respiratory murmur was observed in every case in one or other of the lungs; while over the same lung a clear sound was obtained on percussion.

Bronchial rales of greater or less intensity were heard throughout both lungs in each case.

An impediment to the descent of the morsel existed in three cases, and was referred by the patients to situations nearly corresponding to the sterno-clavicular articulation on the right side.

Second of the General Symptoms.—Pains of a lancinating or burning character around the parietes, or through the centre of the chest, were much complained of in all the cases; they were sometimes remittent, and relieved by pressure.

Dyspnœa or orthopnœa existed in every case. In one only was the dyspnœa inconsiderable.

A cough, usually of a loud, ringing, or croupy character, occurring in paroxysms, existed in each case. In three the

above characters were very remarkable, in one not so much so.

Turgescence of the jugular veins was observed in all the cases. And in two, tenderness of the spine on pressure.

The symptoms which sometimes attend aneurism of the arch, but which were not observed in any of these cases, were as follows :—Numbness, cramps, tingling pains, œdema, or loss or difference of temperature in the upper extremities. Neither was there any difference in the pulse at either wrist.

I am aware that the number of cases I have been able to bring forward is too limited for the purpose of obtaining such general results with respect to the signs and symptoms attendant on aneurisms developed in the thorax, as to be extensively available for practical purposes. I am induced to hope, however, that as far as the arch of the aorta is concerned, the record of these cases may be useful. The diagnosis of the disease, when situated in other portions of the vessel, is still involved in much difficulty ; and although it has received valuable illustration from Bertin, Hope, and Stokes, much, I am convinced, remains for further investigation. I shall continue to register the facts, which additional opportunities of studying the phenomena in these cases may afford me, and shall not again advert to the subject, till they may have so far accumulated as to permit of inductions based on an extensive series of observations.

Art. XXI.—*Contributions to Practical Medicine.* By Dr. Peez of Wiesbaden.

I.

I have lately discovered an internal specific against warts and similar excrescences on the face, hands, and other parts of the body. These excrescenses may be either isolated

and single, or they may form confluent masses, as may be observed frequently on the hands and face. In from four to six weeks, these parasitic growths (provided they have no syphilitic character) invariably fall off, having previously dried up and shrivelled. This specific (for it succeeded in every case which came before Dr. Peez) is the carbonas magnesiæ, of which he gives a teaspoonful daily, in the morning and evening. He thinks it necessary to observe, that none of the individuals, treated by him for the foregoing affections, suffered from acidity of stomach. There are, doubtlessly, other practical advantages of an interesting nature connected with the newly discovered healing power of this remedy.

II.

The host of remedies recommended for the cure of epilepsy is often found to fail in clinical practice, because epilepsy is a disease which, leaving the cause of its origin out of view, exhibits a very great difference in the seat or point of its development. Anti-epileptic remedies have been, generally speaking, employed in practice without sufficient attention to this important point, and hence seldom succeed in curing this serious affection. Epilepsy may be divided into cerebral, thoracic, abdominal, and spinal; perhaps, we may also have a fifth species, which may be termed the mixed. This division is of great importance in practice, for it simplifies our views of the nature of individual cases, and leads to a better choice of remedial agents. The radix pyrethri has been known for centuries as an anti-epileptic remedy; but it was only since I became acquainted with that form of epilepsy which may be termed abdominal, that I have been able to effect cures with this powerful remedial agent. This form manifests itself chiefly in this circumstance,—*the patient, while in a state of perfect consciousness, feels the aura epileptic ascending*

from the solar plexus towards the chest and head; and after he observes this, loses consciousness. In such cases the radix pyrethri acts as a true specific, and cures the patient in a short time. I give it in doses gradually increased to two drachms in the day, and usually in substance. It must be continued for six or eight weeks.

III.

A child, during the period of first dentition, was attacked with diarrhœa, the violence of which was somewhat abated by remedies prescribed by the family physician, but it still recurred with increased intensity. The child was treated for about a year by various physicians of high reputation, and took all kinds of astringent, narcotic, and antacid remedies, and was ultimately given up as incurable. The parents, in deep distress, next applied to me; and on examining the child, who had been now two years ill, I found him in the following condition. He was born of very healthy parents; and for the space of nearly a year had been thriving and strong. The diarrhœa had at the commencement all the characters of diarrhœa from dentition, and was diminished by medicine, particularly by the use of narcotics, which were finally given in large doses to the great injury of the child. He now began to emaciate; and when I saw him for the first time, he had become quite a skeleton. His features resembled those of an old man; his eyes were large and hollow; his voice hoarse: an enormously swelled belly, and extremely emaciated limbs, confirmed at once the idea that the child was labouring under glandular atrophy. His appetite had been long since gone, and it was only with difficulty that he could be prevailed on to take a little mucilage in the shape of drink. A morsel of potato was the only solid substance which he sometimes swallowed, and this was passed half an hour afterwards quite unchanged. On the other hand, his thirst was insatiable; he constantly cried for drink,

which consisted of coffee, and was eagerly swallowed in large quantities. What he did not throw up immediately, was discharged in a short time *per anum*, generally in the form of a colourless and inodorous fluid. He had, in particular, innumerable colourless discharges from the bowels in the twenty-four hours, and it was only very rarely that a small portion of thin, coloured excrement was passed. On examining the functions of the skin and kidneys, I found that for some months neither the cutaneous transpiration nor any discharge of urine had been observed. Under such circumstances, it was natural to expect excessive prostration of strength; the pulse was frequent and small, the extremities generally cool.

From these very characteristic symptoms, I was convinced that in this case the functions of exhalation and absorption were so completely abolished, that the pathological condition present was the opposite of diarrhœa, and that it was rather to be looked upon as a complete paralysis of the vascular terminations of the intestinal canal (*argioplegia canalis intestinalis.*) I was immediately convinced that according to this view of the nature of the disease, calomel was the only remedy by which it was to be met. I gave that evening a grain combined with a little sugar. Next morning the mother told me that the child had had much tranquil sleep during the night, had drank much less, and even had passed some urine.

Second day. A grain of calomel as before: less thirst; evident desire for food; a considerable quantity of urine passed; alvine discharges fewer, and now coloured, and with a stronger smell.

Third day. A very good night; the child only awaked once to take some drink; a grain of calomel in the morning as before; great desire for food, which was eagerly swallowed; thirst gone; alvine evacuations of a greenish-gray colour, and very strong odour; skin somewhat moist about

the neck. On each of the two following days half a grain of calomel was administered. The functions now returned to their normal state, and it became as difficult to prevent the child from eating too much, as it had been previously to check his insatiable desire for drinking. He became quite convalescent, and gave up the use of all medicines.

I have described here the acme of a condition, which is not unfrequently met with in practice even in this melancholy state of intensity, but is much oftener observed in its middle stage; namely, where a portion of the intestinal tube labours under excessive secretion, the function of absorption being abolished. In the last case, calomel in very small doses, with the addition of a minute portion of opium, affords the most ready means of cure. That so many children suffer a long time from disease of the intestinal canal, appears chiefly attributable to this cause; the true nature of the disease is overlooked, and hence the errors observed in treatment.

IV.

In Germany, the treatment of chronic diseases affecting the more cultivated and wealthy classes has taken a more uniform direction. Four-fifths of the chronic cases, in which medicines were formerly employed, are now treated with the medicinal springs. Is is true that for several years back gout and rheumatism, with their manifold products and consequences; as for instance, paralysis, contractions, and swellings of the limbs, herpetic eruptions, and other affections arising from the same sources, have been sent to the hot springs, and particularly to Wiesbaden; and that diseases depending on adynamia and constitutional debility have been recognized as cases in which chalybeate waters are indicated; yet, until a recent period, only a few of the following extensive list of maladies, were recommended to Carlsbad, Wiesbaden, and Marienbad.

The common origin of this class of diseases, according to the views adopted in Germany, is what has been termed, *plethora abdominalis*, (partial congestion, and hyperœmia in the abdominal organs, sluggish abdominal circulation.) To explain the meaning and nature of this condition, is the office of special pathology.

This pathological condition is developed generally betweeen the thirtieth and fiftieth year, as the consequence of free living and hereditary indisposition. In the first stage of its development, it is generally combined with dyspeptic symptoms, acidity, spasms of the stomach, costiveness, and symptoms of cerebral congestion. In Germany, Poland, and Russia, this state is generally distinguished by a tendency to hæmorrhoids, or by an actual periodic hæmorrhoidal flux, which, before it becomes established, is frequently attended with hypochondriacal uneasiness of mind. In this state, so pregnant with alarming consequences, the use of deobstruent medicinal waters, and those of Wiesbaden in particular, affords the most rapid and certain relief. Should the patient neglect to apply for relief, and the disease be allowed to progress, congestion of those abdominal organs which have a larger supply of blood takes place, (as, for instance, of the liver and spleen,) depositions are formed in the omentum and mesentery; the belly becomes swollen and hard; the countenance sometimes assumes a deep red flush, while the extremities emaciate. Many persons are attacked with periodic catarrh, which is not amenable to the usual remedies; others have frequent vomiting, and even hæmatemesis, or periodic headach of a most violent character, and threatening dangerous inflammation, or confusion of ideas, with tendency to vertigo, &c.

Temperament, mode of life, and other influences, give a direction and character to the evolution of these affections. In persons of a sanguineous temperament, intense hæmor-

rhoidal phenomena frequently appear. In others, the foregoing symptoms are relieved, not by hæmorrhoidal effusions, but by a sanguineo-serous diarrhœa, (*fluxus hepaticus.*) In persons of phlegmatic constitutions, the hæmorrhoidal affections alternate with gouty attacks, during which the hæmorrhoids are absent, and again return after the disappearance of the gouty symptoms. Matters frequently go on in this way for some years, unless the external and internal use of some appropriate medicinal spring eradicates the evil, and with it a host of accompanying diseases. It may not be amiss to allude here to some of the latter, and particularly to stiffness of the joints, almost permanent pains, (as for instance, tic douloureux, which is always cured here, unless where it is the consequence of scabies,) paralysis and wasting of the limbs, apoplectic seizures, anasarca of the lower extremities, hæmaturia, sympathetic affections of the heart, (which I have seen cured in this place in different stages, from periodic palpitations to attacks of syncope,) asthma, herpetic eruptions, and ulcers of the legs, &c. Over these forms of disease, sprung from a common source, the waters of Wiesbaden exercise a curative influence. Occasionally, in very chronic cases, the use of the baths and medicinal springs must be repeated. Where the process of morbid development, which has its origin in the abdominal cavity, is allowed to go on, the incurable stage commences. Effusions of serous fluid into the thorax, tumours, thickening of the coats of the vessels, arthritic depositions, ossification of the vessels, induration of the liver and spleen, with disorganization, are the consequences of this state. Yet even in this stage, great relief is obtained by the use of the waters of Wiesbaden, particularly the medicinal spring, for at this period the baths are generally prejudicial.

The object of the foregoing notice is, in the first place, to point out briefly the direction which the healing art has

taken in Germany within the last few years; and, in the next place, to describe more exactly the therapeutic value of the medicinal springs at Weisbaden.

Wiesbaden is by far the most frequented medicinal spring in Germany; it generally contains from twelve to fourteen thousand invalids; and from fifteen to eighteen thousand persons make a longer or shorter stay in it, attracted by the splendour of the surrounding scenery, and the pleasant and agreeable society it affords. It is, at present, the only one in Germany visited during the winter season. Many delicate persons from the north select it as their winter station, its climate being the mildest in Germany, (for instance, it is the only place in Germany where the fig bears fruit in the open air,) and its situation protecting it from the north and east winds. The arrangements for bathing and drinking the waters during winter are extremely well organized.

ART. XXII.—*Letter from* Dr. PERRY *on Typhous Fever.*

Glasgow Royal Infirmary, Oct. 1, 1836.

DEAR SIR,

I observed in the last Number of the Dublin Journal of Medical Science, a letter from Dr. Lombard of Geneva, giving some account of his observations on Typhous Fever in Glasgow, Dublin, and other places; in which letter, though not named, reference is made to my statements and opinions on typhous fever. During the meeting of the British Association in Dublin, it will, I believe, be in your remembrance, my having, on the morning I had the honor of breakfasting with you and other members of the Association at the Lying-in Hospital, shewn you a paper on typhus, in the form of propositions, which, I explained to you at the time, was drawn up at the

request of a commission appointed by the Glasgow Medical Society, to investigate the facts upon which the statements were founded. After looking over this paper, you advised that it should be read that day at the meeting of the medical section, which through your kindness was accordingly done. From my being unfortunately absent at the time the paper was read, it was unaccompanied by those explanatory observations which were necessary to account for the form in which it appeared, and to make it fully intelligible. From this cause, any notice of it which appeared in the periodicals of the day were very incorrect. I, therefore, judged it proper to publish the propositions, with a few explanatory observations, in the Edinburgh Medical and Surgical Journal, for January, 1836.

When I had the pleasure of seeing Dr. Lombard in Glasgow, I was delighted with the interest he took in typhous fever; and during our interview I communicated to him the result of some of my observations on this disease, which appeared to him to be new, and I referred him for others, to the paper above mentioned in the Edinburgh Medical and Surgical Journal. He seemed, from what he heard and saw, to be satisfied—that a considerable difference existed between the same fevers as they appeared on the Continent and in this country; chiefly, in the greater frequency, in the former, of those affections of the bowels, which by some are thought essential to typhous fever. When I was told by him, that he had never seen a case of typhous fever inspected, in which there was not found extensive disease of the intestinal glands, I was not surprized, because I had heard the same statements repeatedly made by medical men in this country, who had studied part of their course on the Continent, and whose opportunities of inspecting typhous cases in this country, were necessarily more limited than my own. I have inspected several hundreds of cases, and have not confined my observations to

those cases where intestinal disease was suspected, but have, uniformly, had the whole intestinal canal slit up, and the mucous membrane carefully examined; in this way I have often detected intestinal disease, when it was not previously suspected. I was anxious, therefore, to learn the number of cases Dr. Lombard had an opportunity of seeing in the hospital to which he was attached, and the symptoms by which they were characterized. If I remember rightly, his hospital did not admit of more than forty fever cases, and was not always full, so that from our greater number, about 2000 annually, and the nearly unlimited opportunities we enjoy of inspecting all the fatal cases, (and which, for some years, I have never neglected to do,) I was able to speak more confidently on the subject, than his experience warranted him to do, so far as the fevers of this part of the country were concerned.

I not only explained to Dr. Lombard, that this local glandular disease of the mucous surface of the intestines, was, as far as my experience went, not essential to typhus, as supposed by most of the continental pathologists, and by many in this country, but that it frequently existed altogether, independently of contagious typhus, and was easily distinguished from it by the absence of symptoms essential to typhus, and by the presence of others not essential to that disease. From the circumstance of this disease of the follicular, or mucous glands of the intestines, being so frequently found in combination with contagious typhus, or following as a sequela of this disease, the distinctions between this intestinal disease and typhus, have been too frequently overlooked, and the two diseases confounded; in the same way that Dr. Armstrong, from seeing the lining membrane of the bronchial tubes so frequently inflamed and thickened, considered these as constant and necessary accompaniments of typhous fever. Dr. Clutterbuck committed a similar mistake, when he concluded, from

seeing the head frequently affected, and, on post mortem examination, the brain and its membranes often exhibiting marks of inflammatory action, that typhous fever was caused by inflammation of the brain. It is true, that the doctrines of Broussais, which ascribe all fevers to intestinal affections are now insisted on by few: but still, they influence the opinions of many, to a greater extent than facts will warrant. In the same way, I have myself seen bronchitis, &c. mistaken by experienced practitioners for typhus. It is hasty conclusions like these, which retard the advance of medical science.

In the paper already referred to, I have mentioned the symptoms which distinguish the one disease from the other. The distinction is not always so broadly marked as there stated. And, I am inclined to believe, that it has been the compounding of these diseases together, which has given rise to the opinion, that the typhous fever of this country is more contagious than it is on the Continent; and also, that to this source is to be ascribed the contrariety of opinion which prevails, both in this country and on the Continent, respecting contagious typhus, which, as far as my experience goes, is a contagious disease, while the intestinal affection is not. Dr. Lombard, towards the conclusion of his first letter, approaches near to what I conceive to be the true solution of the difficulty, when he asks the question, "If we may not infer, that various causes serve to impress upon the general disease (viz. typhus), a tendency to associate itself with, and produce various local ailments?" Among these he enumerates climate, season, diet, lodging, difference of race, and peculiarity of constitution. In his second letter, where he wishes to make out typhus to be, peculiarly, an Irish disease, he appears to have forgot what he had said in his first letter, and is not, altogether, consistent with himself. Nor is it altogether consistent, to call this an Irish disease, and yet call it contagious typhus,

the "fièvre des armes, ou fièvre des prisons." Dr. Lombard, however, is in the fair way of arriving at the truth, and, with so active and intelligent a mind, I have little doubt he will soon arrive at more just conclusions, respecting fever, than he has hitherto arrived at; and, considering the prejudices and prepossessions under which he laboured, the advance he has lately made is highly creditable to his candour, judgment, and love of truth.

A notion has been lately broached, and eagerly caught at by those members of the profession, who find, that they can no longer resist the evidence of their senses, and are yet unwilling to admit, that they have hitherto overlooked some of the most striking characters of contagious typhus, viz: that the typhus now prevalent is a different disease from that which prevailed some twenty or thirty years ago; or, at least, that it is not characterized by the same symptoms, and that it has undergone a modification, or assumed its present character, since the advent of Asiatic cholera. To shew the unsoundness of this opinion, it is only necessary to refer to the descriptions of the older authors, and more particularly, to the very excellent descriptions of Drs. Barker and Cheyne of Dublin, in their reports of contagious typhus. In these, we find exactly the same symptoms mentioned, which characterize the typhus of the present day; and we have, at the same time, the testimony of many of the other reporters, who had seen the disease at former periods when epidemic, that it was in all respects the same.

I have, for some years, entertained the opinion, founded upon an extensive series of observations, that contagious typhus is an *exanthematous disease,* and is subject to all the laws of the other exanthemata;* that, as a general rule, it

* In proof of this, I have often, in cases shewing symptoms of typhus, and when I learned that they had been exposed to contagion, and had not been secured

is only taken once in a life-time, and that a second attack of typhus does not occur more frequently than a second attack of small-pox, and, judging from my own experience, less frequently than a second attack of measles, or scarlet fever.

It is a very generally received opinion, that typhous fever may originate spontaneously, and become contagious from filth, improper diet, impure air, from the confinement and crowding together of human beings, or from some peculiar constitution of the atmosphere, or emanation from the bowels of the earth. Where are the well attested facts to be found, which can warrant any reliance on such fancies—fancies so contrary to sound philosophy? To suppose that any of these causes could generate a specific poison, and this always of the same kind and character, capable of propagating itself by contagion, requires stronger faith than I am possessed of; nothing but the most positive evidence could make me for a moment entertain such an opinion. It is, undoubtedly, a matter of great practical importance, to ascertain exactly the period of the disease at which typhous fever becomes contagious. From numerous observations and experiments I am satisfied, that it is not contagious before the ninth day, perhaps not till a later period of the disease. Among many circumstances which establish this opinion, I may mention one experiment which I made upon a pretty extensive scale. The fever wards of the Glasgow Royal Infirmary are each capable of containing twenty patients. The beds are arranged in two opposite rows, and are pretty near each other. While the patients are in the acute wards, they are not allowed the use of their clothes, though they may be able to sit up; they are, therefore, al-

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against by a previous attack, predicted it with unfailing certainty, the appearance of the typhous eruption on the fifth, or at furthest, the sixth day from the commencement of the attack.

most constantly confined to bed, excepting when rising to stool; and there is about one close-stool to every three patients. Into the fever house are admitted, cases of measles, scarlet fever, and small-pox; and patients are very frequently sent in, labouring under bronchitis, pneumonia, erysipelas, and other local inflammatory affections. I found by experience, that when the latter class of patients were sent to the convalescent ward, where they necessarily mixed with the others, almost all those who had not a previous attack of typhous fever were either seized with it before leaving the house, or returned soon after their dismissal, labouring under it; the period intervening between the time of their being sent to the convalescent ward, and the attack, never being less than eight days. Although means were taken to keep those recovering from small-pox, scarlatina, &c., in a separate room from those convalescent from typhus, the rooms being adjoining, the non-intercourse was incomplete, and the result was, that these diseases occasionally spread among the typhous convalescents, and the convalescents from small-pox and scarlatina caught typhus. In consequence of these observations, I adopted the practice of not sending, as formerly, to the convalescent wards, those patients affected with inflammatory diseases, unless I ascertained that they were secured against the disease by having had a previous attack of typhus; but kept them in the acute fever wards, till they were so far recovered as to go to their own houses, and the result was, (and the practice was continued for several months,) that not one of those detained in the acute wards caught the disease while there, or returned with it afterwards. From the above, and other observations, I have adopted the opinion, that typhus, like measles, small-pox, &c., is chiefly spread during the period of convalescence. In the paper already noticed, I have mentioned the desquamation of the cuticle, which usually takes place when a patient is convalescent from typhus. Do the

fine scales thrown off in this state contain the poison, which, by adhering to the clothes and hair of the patient, are carried about with him, and being rubbed off, are, while floating in the atmosphere, applied to the mucous surface, or inhaled by a susceptible recipient, in whom it produces, after a certain time, the specific disease? Whether this is the way the disease is spread or not, further observations must prove. The practice of shaving the head of every *fever patient* admitted into the Glasgow Royal Infirmary is a good one, and ought to be universally adopted, as well as the practice of causing every such patient, before dismissal, to be well washed in a tepid or warm bath. I may mention that I am, at present, engaged in preparing statistical tables to illustrate some of the points here adverted to, and others connected with fever.

Permit me, in conclusion, to return my sincere thanks to you and the other medical gentlemen of Dublin, for the very kind and friendly manner in which you treated the gentlemen from Glasgow, who attended the meeting of the British Association. All of us remember it with pleasure, and, for myself, I state it seriously, I think it among the happiest periods of my professional life. I trust the gentlemen of Glasgow will soon have an opportunity of returning the compliment.

Believe me, my dear Sir,

Yours sincerely,

ROBERT PERRY, M.D.

ART. XXIII.—*Cases of Placenta Presentation.* By WILLIAM JAMESON, M.D., Member of the Royal College of Surgeons of Ireland, and Demonstrator of Anatomy to the Dublin School of Surgery and Medicine, Digges'-street.

NOTWITHSTANDING the subject of placenta presentation has been so ably treated on by so many authors, I conceive that the following cases, together with the practical inductions they have given rise to, will not be deemed undeserving the attention of the profession.

In the month of October last, I was called to Mrs. T——, then in labour of her ninth child; she had been ill about two hours before I saw her, and each pain was attended with a discharge of blood from the vagina. On examination I found the os uteri dilated to the size of a half crown, and a small lip-like substance placed between my finger and the head of the child, which I at once recognized to be a portion of the placenta. The discharge not having been such as to weaken my patient, I determined to watch her closely, and not interfere unless I found it absolutely necessary. As, however, labour advanced, each pain was attended with a gush of blood; but still my patient was strong, and did not appear constitutionally to to suffer from its loss, although three pints might have escaped in the space of two hours, at which time, on further examination, I found that the os had dilated to the size of a crown piece, the membranes protruding beyond the edge of the placenta, and the head of the child to be felt.

Under these circumstances, I ruptured the membranes, when immediately the head came down, and pressing on the superior strait of the pelvis, completely arrested all further hæmorrhage.

Labour went on regularly; and in about one hour more

the child was born, followed by the placenta in about ten minutes afterwards. From this, the case went on most favourably; and this lady recovered without any unfavourable symptoms.

Reflecting on the above case, it occurred to me that the rule *almost invariably* laid down by authors on the practice of midwifery, as to the necessity of turning the child, and delivering by the feet in every case of placenta presentation, admitted of being very much questioned. And that by expediting the labour in the manner resorted to in this case, all the advantages to be obtained by that practice might be attained; at least that cases might occasionally present themselves in which it could be used with every success, and this operation avoided. Accordingly I was determined on the first opportunity to try this plan; and it was some time before such was afforded me. However, on the 29th of July last I was sent for to see Mrs. S——; residing some short distance from town, I arrived at about eight o'clock in the morning; and found my patient (who did not expect to be confined till the following month) to all appearance dead, having no pulse perceptible at the wrist, her face perfectly pallid; and the whole surface of the body covered with a cold, clammy sweat.

As this lady's confinement was unexpected, she had no nursetender with her; and on inquiry from her servant, I was informed that her mistress had gotten up to the night-chair at about six o'clock, where she became very weak and exhausted from loss of blood; that on being placed in bed, labour pains came on, attended with an immense discharge of blood. I found her swathed with three sheets, perfectly saturated; and although lying on a feather bed and pallias, the blood had filtered through both, and was streaming on the floor.

In this appalling case I lost no time in making an examination. The vagina I found full of clots; the os uteri

dilated to the size of a half crown, and very rigid, with the placenta presenting; the edge of which I could not reach with my finger; and although she lay in a state of syncope, and the uterus by no means in action, the blood was still draining from her.

I immediately gave her a tumbler full of equal parts of brandy and water, which I got her to swallow with great difficulty, and by this means having roused her a little, I removed all the wet sheets, and plugged the vagina with a towel wet with vinegar and water. I then explained to her husband the very precarious state in which his lady was in, and that I feared it would be necessary to turn the child, in order to save her life; upon which, further assistance being suggested by him, I gladly acceded to it, and had my friend Dr. Shackleton sent for; during the interval that elapsed between his being sent for and arrival, I continued the brandy and water in such quantities as I judged necessary, and as the plug restrained all external hæmorrhage, the pulse got up, warmth was generated over the body, and the countenance was better by the time Dr. Shackleton arrived, and as the labour pains were increasing in force and frequency, it was agreed in consultation to give twenty drops of tincture of opium. I removed the plug to ascertain what progress labour had made, in order that if turning would have been deemed necessary, we should proceed with it. In doing so I found that the os uteri had dilated, and that the placenta was every where adherent to its edge; and with some difficulty, succeeded in getting my finger beyond its edge, towards the anterior part, and felt the membranes tense, and the head of the child above.

Under these circumstances, I determined on rupturing the membranes, and ascertaining whether this would arrest further hæmorrhage, (which was necessarily increased during the examination,) but found them so tough and rigid, that I was obliged to withdraw my hand; and at the

suggestion of Dr. S., cut a sharp point on the nail of my index finger, with which I succeeded on the next attempt. The waters were discharged; and the head immediately fell down a little below the superior strait, pushed the placenta to one side, and completely arrested all further hæmorrhage; in so much that I did not deem it necessary to trespass on my friend's time any longer; and in about two hours afterwards she was delivered of a male child (which appeared to have been two days dead) of an unusually large size, considering it to have been but an eight month fœtus: but this lady's children were always large at birth.

I have been induced to give publicity to these two cases, as they plainly shew that turning is not invariably necessary in every case of placenta presentation; and as that operation, let it be ever so dexterously performed, is not always unattended with danger, it ought, consequently, to be avoided in all cases, if possible; particularly, if on more extensive trial, the method above employed will be found to succeed.

ART. XXIV.—*Pathological Contributions.*—No. I. By ALEXANDER JOHN HANNY, Professor of Physic, Anderson's University, Glasgow; formerly Senior President of the Royal Medical Society of Edinburgh, &c.

CASE I.—*Ulceration of the Brain.*

ANY eulogium on the vast advantages to be derived from careful notice of the symptoms of disease during life, and minute attention to the relative changes effected on the organization—as seen on the inspection of the dead body—is in these times uncalled for and preposterous, for it stands universally confessed, that by observation, and judicious comparison of living phenomena with *post mortem* appearances, a progressive impulse has been given to pathology, and many signal improvements achieved in this branch

of knowledge. But I wish to impress on the younger members of the profession, that whilst, by the *careful* study of disease in the living body, and of the effects of it in the dead, the science in *general* has been advanced—this method is the only sure path to *individual* and personal attainment. And though our labours may not be crowned with brilliant discoveries, yet I deem this the most invigorating training for one destined to combat disease, the most effectual, nay, the only method of obtaining an exact and useful acquaintance with those truths, a knowledge of which fits the pathogist in the highest degree for the practice of medicine. Indeed, I do not hesitate to say, that the first exercise of the noviciate in physic should be the *observation* of *disease ;* it may then, of course, be best conducted under the eye of the master; but the student cannot, I maintain, apply himself too early to the observation of disease. What he sees and learns in this exercise gives a deep interest to those studies called elementary. I have invariably seen (and I have taken pains, as well as had very ample opportunities to observe) that the young men, who had been accustomed to this practice, who knew the external manifestations of morbid action, and of organic disease, take the deepest interest in anatomy and physiology. To them the advantages of anatomicalknowledge are most apparent ; they give it their most devoted attention, because it throws light on this or the other disease, or morbid condition, with which their observation had rendered them familiar; to them the prelections of the anatomical and physiological teacher are invested with an interest that warms them into ardour and enthusiasm in the prosecution of their studies. Consequently, the progress of such persons is infinitely more rapid, and their attainments in the same period more complete, than in those who did not enjoy the like advantages. A very opposite method is too generally followed in our schools, to so great an extent, and with influences so injurious to students, that

I would beg for a little to draw attention to it. A tyro, as his first step, engages in the study of anatomy ; he attends the lectures from a sense of duty, or authority compels; but he does not discover, or appreciate, the benefits to be derived from it; he may be told, but he does not experimentally know its important applications to disease, and to the practice of the art; and having but little interest in what he does not fully value, he loses a great part of the benefit of attendance, there being little to fix on his mind the many facts displayed in a course of anatomy. Indeed, it is commonly two or three sessions before he attends lectures in which the phenomena of disease are discovered and explained to him. All this time he has been attending, often in the most lifeless and uninterested manner, the courses of anatomy required by the curriculum of education prescribed by the institution from which he wishes to obtain his authority to practise. At length, with a very imperfect knowledge of anatomy, he comes to the study of the symptoms of disease, and the theories advanced to explain them. He now sees his deficiencies in anatomy, and finds out the deep interest that would have riveted him to many parts of the economy, had he only known the practical application of this acquirement. I ask if this have not been the experience of many, that on coming to the physiological, the pathological, and more especially to the surgical class, they have first found their deficiencies, and duly estimated a profound knowledge of anatomy? Some can remedy the defect at this period. But on the other hand, to many this discovery of their deficiencies comes too late ; their curriculum is finished; all the classes have been *feed ;* and the purse-strings of parents and guardians are closed ; and they *must* make a finish. I again repeat that all this might have been prevented, if the pupils were taught early to study disease ; *to note down* its symptoms and progress ; and to witness inspections after death. Besides by such

exercises, and by keeping *a written record* of them, he not only acquires knowledge of the highest importance, but he is learning also to express his thoughts in words. The very act of minutely describing a case, implies careful observation, enforces diligent study, and begets habits of attention. I beg then to urge on the younger members of the profession, even the youngest, to commence the practice in question as early as possible; a practice which has advanced the *science*, and cannot fail to improve every *individual* who sets himself to it in good earnest. It is probable that many pursue this method, and reap its sure advantage, of whom I know nothing; but I know many who do not practise this as they should, only making slight, capricious, and partial notes; never an accurate observation of every function, and a full detail of every morbid phenomenon; which neglect I have often seen regretted when too late: that was at the exposure, by dissection, of some important change of morbid structure or lesion, the existence of which they never once suspected; though such could have been foreseen by the superior care and sagacity of others, or by their own more attentive observation.

To encourage the young observer in his record of disease, I would further hold out to him, that the benefits of the practice in question may not always be confined to himself, great though these undoubtedly are, for the ingenuity, address, and application of another may have discovered what the observation of an ordinary or inexperienced observer shall confirm, or confute, which last is sometimes no inconsiderable service; and for having added something to the stock of useful knowledge, the labours of even the youthful observer may be crowned with approbation and honourable distinction,—the gratifying rewards of every generous mind. On the whole, the diligent, steady, and industrious observer may rely with certainty, that though the results of his earliest essays should be of little intrinsic value, yet they

are inestimable as exercises leading to individual improvement in observation and *description.*

Under these impressions I have collected many cases, and beg permission to record, from time to time, in your Journal, such of them as appear to me worthy of being preserved, with a view, either now or at a future period, of confirming some principle which has been proposed, or of shewing some that have been received as true, to be fallacious; the cases are mostly isolated, and, with a few exceptions, seldom have any thing in common, except that of occurring in the same cavity. The first case is an addition to the few we have on record of ULCERATION OF THE BRAIN; I am induced to give this case at all the length I recorded it in my case book, and almost in the very words used there, as I have, on reading a case, often wished to interrogate the observer a little more concerning certain points which he had omitted in the description, or merely noticed in a loose and inaccurate manner. I have often heard it said that we ought not to encumber the record of cases with tedious detail; that we should be brief and spirited; but whilst I reprobate the introduction of extraneous matter, or too many general discussions into cases, I believe it possible to be too meagre in our observation and detail of phenomena; and though I admit brevity to be the soul of wit, I cannot allow it to be equally essential in scientific narration; for it seems to be a great error to omit any fact, the existence of which is well ascertained, merely because we do not perceive its utility or importance. If facts be correctly stated, they can hardly be recorded too minutely, and a future age may derive instruction from that which, to the original observer, seemed of little use.* Lastly, in this apology I would add, that "when the comfort, the health, and the lives of mankind are concerned, too much care cannot be bestowed on writing or

* Dr. Cleghorn's letter to Dr. Watt, *vide* Watt's Cases of Diabetes.

perusing the history of disease."* The following account of a case contains all that I deemed worthy of observation, and indeed all that was noted:

Case I.—Alexander Gilmour, æt. 9, 22nd Feb., 1829, a spirited, active boy, but thin, and of an unhealthy aspect. Has violent pain in forehead and heat of scalp; is pale, downcast and oppressed; pains dart suddenly through his head from ear to ear, and make him scream; he has had a most distressing night from this cause. He always applies his left hand to his brow. Nothing particular observable about his eyes; sickness and vomiting. Febrile heat and rapid pulse, 116; bowels open; tongue furred; thirst. Has for some time past been very "cold-rife," creeping close to the fire; and last night he had a slight rigor. He has been attending school regularly till three days ago. When an infant he received a severe blow on the occiput, but it has not troubled him since; and no external marks of the injury are apparent. He has long laboured under a severe cough, and occasional pain of chest, which followed an attack of pertussis. Has had a saline purgative this morning.

Admov. Hirud. vi. utrique tempori, postea Lotiones frigid. capil. prius abraso. Habt. Tart. Antim. $\frac{1}{4}$ gr. om. hora ex aq.

8. P. M. Leeches bled indifferently, but head easier; extraordinary oppression indicated by frequent and deep sighs; countenance expressive of distress, pulse slower (96;) solution has made him vomit occasionally.

Contr. Lotiones frigid. necnon solutio sed in dimidia portione tantum.

23rd. During the night the pain became so agonizing that they gave him ten drops of laudanum. Towards morning he fell asleep; says he has no pain of head this morning, though still it is very hot. Pulse slow, only 68. Lan-

* Watt, *l. c.*

guid : sighing and oppression still continue : tongue clean and moist. Complains that he sees red streaks on the wall in the dark ; though not at present. He has taken nine spoonfuls of the antimonial solution, containing in all two grains and a quarter. Vomited after each spoonful until the sixth, when *tolerance* seemed to be effected. No stool or sweating.

Admov. vesicator. occip. et nuchae Hirud. vi. utrique tempor. prius admotis. Ht. Sulp. Magnes. ℥ss. Contr. Lotion. et Sol. 2da q. q. horâ.

24th. Blister was not applied, his friends being unwilling to "*torture*" him, but leeches bled well; no vomiting; no stool ; pulse still slow ; pain of head severe ; delirium.

Admoveatur vesicat. nuch. et occip. Contr. Lot. frigid. fronti. Foveantur crura. Repet. Sulp. Magnes.

25th. Took two doses of the epsom salts; little movements, blister rose but badly. Symptoms exactly as yesterday.

Admov. Hirud. xii. Habt. pil. Ex. Calom. gi. iv. et Opii g. i. s. et Infus. Sennae cyathum C. P. M.

26th. Leeches bled well ; no relief ; pain as before ; medicines operated four times : tongue furred ; pulse 28.

Hirud. x. utrique temp. et vesicat. summo capiti. Ht. pil. Calom. g. ii. et Opii. gr. ii. ss. 3d q. q. horâ.

27th. 10 A. M. Lay quiet for a part of the night, after four doses of calomel and opium ; but complains now of much pain in head, and of noises as if a piano were jingling in the room ; great distress ; pulse wiry, quick, and regular ; eyes still unaffected ; delirious occasionally ; thirst ; heat of surface ; pain of head aggravated by motion. On opening the jugular vein on each side, no blood flowed. From the temporal artery eight ounces were got very quickly. The pulse reduced in force but not in frequency. Says pain of head is less ; pale, and lies quite exhausted looking ;

pupil of right eye is irregular, and dilated on the light of a candle being cast on it; the left eye is natural; pain now referred exclusively to the left side of the head. Both previous blisters have risen badly; no stool; no vomiting.

Admov. vesicat. toto capiti et contr. Calom. et Opium.

28th. He gradually became weaker, and expired without a struggle or convulsion at 3 P. M.

Dissection, on 2nd March, 2 P.M.—Body much emaciated, and very pale; few vessels bled on cutting the scalp; muscular fibre very red; calvarium thick, and very completely ossified; no spiculæ; no mark of the blow he received in earlier life; longitudinal and other sinuses very full of a dark, grumous blood. The veins of the membranes in a similar state of distention. Many vessels bled profusely on tearing off the skull-cap from the dura mater. This membrane was lacerated, and allowed the escape of a small quantity of serum collected between it and the arachnoid. The quantity of serum here, or in any part, was not much, if at all, more than naturally found; that in ventricles was slightly tinged with blood. The arachnoid was opaque, and had a red tinge from the pia mater underneath being very vascular, and of a bright red colour. One or two small patches of coagulable lymph were seen on the pia mater; one on the left lobe of the cerebellum, under the tentorium; another on the under surface of the left middle lobe. They presented a ramified appearance, being deposited in the course of the large vessels. The membrane at these places appeared thickened, and the substance of the brain immediately underneath felt decidedly softer, more pulpy than at any other part, but, with these exceptions, it had a natural degree of firmness. The surface of a section of the brain presented a more than ordinary number of bloody points.

On the surface of the left anterior lobe, on its outer and

under side, there was a cavity or excavation of the convolutions, partly smooth, partly granulated, of a pale primrose hue on some parts, and of the cineritious colour of the brain in others. The granulations were small, whitish, shining bodies, and gave to the surface of the cavity the appearance of being sprinkled over with oatmeal in a moist state. Over part of the surface there was a cobweb-like, expansion of small vessels: this was principally on the scabrous or granulated part. The cavity was of an irregular oval shape, with an area greater than that of a shilling; its edges were more than an eighth of an inch in depth, and fringed with the membranes which had been destroyed by the ulceration; its surface was as soft as thin paste or starch, but not diffluent.* The corpus callosum, fornix, corpora striata, thalami, and cerebellum, exhibited no traces of disorganization, and except the turgid state of their vessels, appeared to be healthy.

The lungs were universally studded with tubercles; some in the miliary stage, others forming consolidated masses of greater or less extent, and approaching a softened state.

I shall here abridge the account of the appearances in the mucous texture, by saying, that in many parts it presented the ecchymosed or injected state, said to be the *vestigia* of inflammation.

Remarks.—It will be very readily believed, I think, that I never once suspected the existence of ulceration in this boy's brain, until the inspection.

The phenomena observable in this case were certainly the same, in many respects, as occur in cases of inflammation of the brain and its membranes, called hydrocephalus; and it is probable that most of the prominent symptoms of

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* Mr. J. A. Gilfillan, Professor of Painting, of Anderson's University favoured me with a very admirable delineation of this appearance.

this case were actually produced by *phlogosis* of these textures.

The sinking of the pulse might have encouraged the conclusion that the case had proceeded the length of effusion. It will be observed that there is in this case one striking difference from what commonly occurs in cases of effusion from inflammation within the cranium—there was no coma, which generally attends the stage of effusion in meningitis. In those cases of inflammatory affections which proceed the length of effusion, we have coma of some duration as a constant symptom; and dissection has shewn, that in meningitis destroying the patient, without coma having been a symptom, there is generally no effusion.* Had I adverted to this, I think I might have anticipated the absence of effusion.

The appearances of the brain in this case were very characteristic of ulceration of this texture, and did not admit of a doubt as to there being loss of substance effected by a vital process. I was pleased to find that my description of the appearances (though in other words) fully accords with the accurate definition which Dr. Craigie† gives of this process in cerebral structure. "A hollow, or depressed surface; rough, irregular, and covered partially either with bloody or albuminous exudation;" appearances pourtrayed by the terms "*cavity*"—"*granulated and scabrous surface, as soft as starch,*" employed in my description. The records of cases of this nature do not present us with any pathognomonic feature, any symptom by which we can recognize its occurrence. Indeed, I do not find that its existence was anticipated in any one of the cases of which we have descriptions. It is worthy of remark, that there were no convulsions, no rigid contraction of the flexor muscles, nor paralysis, though these were prominent symptoms in most

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* Abercrombie. Case xi. p. 57. † General and Pathological Anatomy.

of the cases detailed to us. In having an inflamed state of the pia mater, and in the brain being softened at other points, it corresponds with the cases reported by other observers, Haller, Stall, Scoutteten.

In the two cases described by Scoutteten, there was severe disease of the alimentary canal; and in the case before us, indications of gastric derangement early presented themselves, and formed an urgent part of the sufferings; distinct and extensive traces of gastro-enteritis appeared on inspection.

Scoutteten is of opinion that in the cases he saw, the inflammatory affection of the brain and membranes was subordinate to the phlegmasia of the alimentary mucous membrane. The case given by Morgagni presented severe abdominal disease.

The intermission of pain which occurred in this case has, I find, been noticed by others in cases of ulceration of the brain. And in one case recorded by Scoutteten, pain of head did not exist.

I have speculated often on the duration of the complaint in this boy, but can come to no very satisfactory conclusion. Most of the cases I have read were of considerable protraction, exhibiting symptoms of cerebral disease for many weeks; and had all the cases been so protracted, I should have allowed analogy to influence me in concluding that my case, previous to assuming a violent form, had been making obscure advances. But in one of Scoutteten's cases, the head symptoms existed for three days only previous to death. I am, therefore, induced to believe, that in this boy the whole mischief was begun and accomplished in a short space of time.

1st. Because other cases countenance the possibility of such short duration.

2ndly. Because I cannot conceive destruction of this nature prevailing without some symptoms indicative of

cerebral disease, not one of which, as far as I can discover after careful investigation, this boy had, until a day or two before his brief but fatal illness.* The only fact which shakes my faith in this argument is one recorded by Sir James Earle, who opened the head of a man that had died, as he supposed, from cachexia, induced by syphilis, and monstrous exhibition of mercury. There was not a suspicion entertained of cerebral disease; not a symptom of cerebral affection existed; yet a large abscess of the brain, and ulceration of the surface of the thalami, (*nervor. opticor.*) were found. This interesting case is recorded in the Medical and Physical Journal, for March, 1810. Still the health of the man was much impaired; he was cachectic. In the subject of my case, there were no very prominent features of disease till the fatal illness.

It is necessary to say in conclusion, that ulceration of the surface of the brain, independent of any other morbid state, of which the case above described is a very unequivocal example, must not be confounded or associated with that disorganization, or loss of substance, which is produced by, or is the consequence of the rupture of a vessel, effusion of blood, abscess, or softening of the brain, and the like. It is possible, too, that the appearance of ulceration may be made by inattention or violence in dissection.

There is an appearance of the surface of the brain produced by violent injuries, as in falls from a considerable height, which looks somewhat like destruction by ulceration. I have been present at an inspection where it was a matter of debate whether the loss of the surface of the brain was the effect of mechanical violence, or produced by ulce-

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* Stoll remarks on a case he narrates, in which ulceration of the cerebellum was found, the patient having been severely afflicted with violent headachs, that the ulcer seemed to be of long standing, "ob antiquos capitis dolores."—*Ratio Medend. de Causa et Sede Phrenit.* vol. ii. p. 229.

ration. Perhaps a few observations which I have collected on the subject may not prove uninteresting, indeed may draw attention to the point. I shall, with permission, make these the subject of my next communication, under the title of "Traumatic Destruction of the Brain's Surface *resembling* Ulceration."

ART. XXV.—*Some Remarks on Epidemic Typhous Fever which prevailed in the Parish of Donoughmore, County Donegal, in March, April, and May,* 1836. By THOMAS HENDERSON BABINGTON, M. B., L. R. C. S. I., Superintendent of the Donoughmore Dispensary.

FOR many years past there has not been in the recollection of the oldest inhabitant of this district, a spring characterized by equal severity of weather, or such sudden variations of climate as the last. Heavy hail and snow showers, succeeded by high, piercing, westerly winds, and cold rains at night, having prevailed during a great part of the season.

Among the poorer classes of the population, devoid of almost every comfort, ill lodged, and scantily supplied with firing, and at the same time exposed to all the inclemencies and vicissitudes of the weather, without sufficient clothing to protect them, and frequently depending on the same covering by night which they had worn during the day; their damp dwellings crowded almost to suffocation, (the only apertures for ventilation being the door and chimney,) with a manure heap, and a pool of filthy, stagnant water generally close to the door; added to which, their diet being almost exclusively vegetable, without being able, in many cases, to procure milk; three meals of potatoes, with sometimes the variety of a salt herring, constituting their sole support, it is natural to expect that disease would soon

make its appearance, and spread with fearful rapidity amongst a class of persons circumstanced as I have mentioned; and what could tend more to the engendering and propagation of typhous fever.

Accordingly, typhous fever was very prevalent in this parish during the months of March, April, and May; the form of the disease, in many particulars, resembling that designated by Dr. Armstrong as the first form of continued typhous fever; with, however, certain variations, which I shall briefly describe as it fell under my observation.

The first stage of the disease had generally passed over before I was called to see the patient; and on making minute inquiries, I generally found that, three or four days before, he had had a rigor or cold shivering, lasting for two days, with langour and lassitude; he continued walking about; was then attacked with general pains; and described his state by saying, "there was not a part of his body free." The bowels were confined; the pulse quick, and full; tongue covered with a dirty white fur; confused pain in the head, generally confined to the frontal region; oppression at the præcordia; flushed face; great prostration of strength; and slight cough denoting derangement of the bronchial membrane; very considerable thirst; great loss of appetite; and pains in all his joints. This stage of the disease was followed by another and different set of symptoms; the bowels having been confined in the former stage; and this constipation having been removed by purgative medicines, the headach ceased; the pulse seemed to be natural; the thirst abated; but on examining the abdomen, very considerable pain was experienced on pressure, and this pain principally over the region of the transverse colon; this was quickly followed by diarrhœa; the pulse became more frequent, being about 100, and frequently weak and tremulous; tongue brown, and glazed; black sordes on the

teeth; lips retracted; skin hot, and pungent; generally disturbed sleep with delirium; and almost in every case, the patient became deaf, as soon as this the third stage of the disease set in. He also had, in this stage, severe cough; and on applying the stethoscope to the chest, all the signs indicating bronchitis were found present. It was at night that most sleep was procured; hence, in the morning there was a considerable alleviation of the symptoms.

The state of the tongue, above mentioned, having been altered, and the diarrhœa checked, or at least moderated, the fourth stage of the disease commenced, in which it often required even weeks to make any decided step towards convalescence. The first symptoms which marked any change for the better were, a slight cessation of the diarrhœa; the tongue at the same time became cleaner and more expanded; a desire for food; and the gradual disappearance of a filthy, clammy look, which before had disfigured the patient's countenance. But in many cases, the diarrhœa, and even some abdominal tenderness, continued throughout this stage.

Let us now briefly notice the treatment had recourse to in the various stages of this disease. And here we may mention that, we are laying no claim to original views; our object is merely to submit to the profession a detailed account of the practice here adopted, which must, at least, be acknowledged to have been successful, when it is considered that out of so many as ninety cases, only five persons died.

In the first stage of the disease, when there was considerable excitement, with headach, full pulse, and loaded tongue, some would conceive that phlebotomy would have been advantageously had recourse to. Such is the practice Doctor Armstrong strongly recommends. *But,* however limited my experience may be, I must beg leave to differ with such respectable authority; the practice of general blood letting is much too frequently resorted to by country

practitioners in fever cases, as well as many other acute diseases. Although the peasantry appear to be full, strong, plethoric, and apparently in robust health, still, when affected with acute diseases, they have not sufficient stamina to bear the copious depletion they are frequently subjected to in the very free abstraction of blood, too often had recourse to by *village practitioners;* an assertion, the truth of which I found exemplified in several cases of the epidemic fever under consideration, where bleeding had had been resorted to previous to my being called to see the persons attacked; these were, subsequently, in much greater danger, and had a much more tedious illness than those who were differently treated. Persons fed on diet almost entirely vegetable, as the population of this district are, do not, in my opinion, bear the free use of the lancet well; hence, the line of practice I adopted was to relieve the loaded state of the bowels by a full dose of calomel, and compound powder of jalap, followed by castor oil; in some cases a repetition of this medicine was necessary, and even in several cases I was obliged to have recourse to *croton oil*,* to remove the constipation; (where the person attacked was seen at the very outset, that is to say, immediately after being affected by the shivering, an emetic, followed by a dose of purgative medicine, seldom failed in checking the disease;) this stage of the disease was further treated with solution of tartarized antimony; and in cases where the heat of the head was very great, the temples were bathed with vinegar and water; by this mode of treatment the stage of excitement was moderated and relieved, but was soon succeeded by other and very different affections, in which an altered line of practice had to be adopted.

* Some may remark that the diarrhœa was caused by the use of so drastic a purgative as croton oil; such, however, is not the case. The croton oil was not used in a greater proportion than in one out of ten cases, whilst the diarrhœa was present in every case.

I mentioned above, that in this, which we may call the second stage of the disease, there was considerable tenderness of the abdomen on pressure, especially over the colon; skin pungently hot; the tongue brown and glazed; the pulse quick and tremulous; and these symptoms accompanied with a distressing diarrhœa, clearly indicating that inflammatory action was going on within the abdomen, to combat which I was induced to adopt the following treatment, viz.—as a topical remedy, turpentine stupes were applied to the abdomen; Hydrarg. c. Creta, and Dover's Powder were exhibited in the proportion of three grains of the former to two of the latter, every fourth hour.* If on again seeing the patient in about twelve hours, the symptoms remained unsubdued; the stupes had vesicated the abdomen; the diarrhœa still continued; no change in tongue or pulse; I directed the stupes and powder to be continued, and to take an ounce of infusion of catechu after each powder; this treatment was persevered in for the next twelve hours, and by it the diarrhœa was in general checked, and the state of the tongue and the intestinal canal much improved. In such cases as this treatment failed in, the disease ran into the third stage, which was ushered in by the patient complaining of dull pain in the occipital region, with deafness; he lay on his back in a fixed position, and appeared to be insensible to all surrounding objects. Here we had evidence of cerebral congestion; and, indeed, in all the cases of fever which came under my observation, I re-

* Some may conceive that that the abstraction of blood by leeches or cupping would have been more judicious practice; however, when they consider the expense of procuring leeches, they will perceive that the funds of country dispensaries cannot afford them. And as to cupping, the time of a dispensary doctor in the country is so taken up with his multifarious duties, he cannot afford to remain so long with his patients as would be necessary for abstracting blood in such a manner, especially in cases of epidemic fever, where, perhaps, fifteen cases are to be visited daily, and these in scattered and detached dwellings.

marked that deafness with pain in the occipital region, are two symptoms which are much to be dreaded. I generally succeeded in removing these affections, by the application of large blisters to the back of the neck.

All the urgent symptoms having been alleviated by the treatment detailed, the state of convalescence seemed to be commencing, generally about the twenty-eighth or thirtieth day, but so very gradual was the change for the better, that it was only by comparing one day with the two or three before, that any marked difference could be detected. The patients now had a voracious appetite, so much so, that it was very difficult to persuade them to be satisfied with small quantities of light food: in several cases where they had opportunities of gratifying their rapacity, they relapsed almost immediately after, and were with great difficulty recovered. In many cases the diarrhœa continued obstinate through a great part of the stage of convalescence; in such, when all other medicines failed, I found very great benefit result from the use of a medicine but seldom ordered; I mean pomegranate powder. I gave it in ten grain doses, three times a day, and in no one instance did it fail; scarcely had three doses been administered when the calls to stool became much less frequent, and in two days the affection was checked, and the medicine discontinued. As a tonic in this stage of convalescence, the precipitated carbonate of iron, in scruple doses, three times a day, was principally relied on; and under its use, it is surprising how soon the patients regained their strength.

I shall conclude these remarks, by briefly noticing an affection of the skin, or rather of the cellular membrane, which was present in nine-tenths of these cases of fever, and which I have not found mentioned by any writer on the subject of fever, viz. the appearance of large furunculi on various parts of the body, but principally on the extremities. I do not know how to account for such an appear-

ance, after typhus fever; the affection was generally removed by a few doses of purgative medicine.

In submitting these remarks to the public, I am making no claim to originality of either views or treatment; I indulge in neither fine drawn speculations nor theories; I merely wish to detail a line of practice, which in my hands has been marked with decided success in the treatment of typhous fever, as it existed in this district during the late spring season; and to conclude, I may not perhaps inaptly add,

> " Scripsi fide medicâ probaque pietate
> ——— si quid novisti rectius istis
> Candidus imperti: si non his utere mecum."
>
> KLEIN, *Interp. Clinic.*

ART. XXVI.—*A Case of circumscribed false Aneurism of the Brachial Artery, caused by Puncture in Blood-letting, and cured by Operation.* Communicated by THOMAS WALL, Member of the Royal College of Surgeons, London; and Resident Surgeon to the Cork General Dispensary and Humane Society.

PATRICK Connell, aged 27 years, of a sanguine temperament, and plethoric habit, by occupation a labourer, and living on the road to Ballincollig, about two miles and a half from this city, was admitted a patient at the Cork General Dispensary on the 11th of August, 1836, suffering from severe head-ach. He gave the following report of his case to his medical attendant Dr. O'Flyn. About five weeks previously he had an attack of fever, and since his convalescence, was troubled with uneasiness in the head. His habits were temperate and regular, and he always enjoyed uninterupted good health up to the period of his late illness. He was treated with purgatives and

tonics, until the 19th, when he directed the doctor's attention to a "lump" at the bend of his right arm, and which he said was gradually increasing, but gave him little or no pain. On inspection, the doctor perceived that it had an ambiguous appearance, and called my attention to the case. At first sight it appeared a circumscribed tumour, about the size of a pigeon's egg, at the bend of the arm, having no discoloration of integument, or inflammatory appearance. After close examination, we discovered a pulsation, which was rendered very evident by the stethoscope, and a cicatrix in the median basilic vein. The nature of the disease was no longer doubtful; pressure on the brachial artery stopped the flow of blood into the aneurismal sac, which could be easily emptied, and refilled with a sudden gush on removing the pressure. The patient then stated that he had been bled in that arm five weeks before, and that the young gentleman who performed the operation found it extremely difficult to stop the bleeding, and likewise, that the venesection had given him very great pain. The doctor having transferred the case to me, I explained to the patient the nature of the accident, the necessity there was of implicitly following my directions respecting rest and regimen, with the almost certainty of an operation; and though a poor uneducated peasant, he unhesitatingly agreed to it, or any other mode of treatment that I thought necessary to adopt. I was anxious to try what local and constitutional remedies could do, and accordingly ordered some purgative medicine, followed up by nauseating doses of tartar emetic, tincture of digitalis, and the recumbent posture. I placed a compress and bandage over the brachial artery and aneurismal sac; having first emptied the latter of its contents. Unavoidable circumstances prevented my seeing him for a few days, when I thought there seemed some decrease in the tumour. I continued

this treatment for eight days, until I perceived considerable vascularity, glossiness, and thinning of the skin covering the tumour. Besides these appearances, the limb was swollen; and the patient declared he could no longer bear the necessary pressure.

On consultation with Doctors Hobart, Townsend, O'Flyn, and Mr. M'Evers, I determined on tying the artery the ensuing day; and having prepared the patient by suitable purgatives, I proceeded with the operation on Saturday, the 27th of August, assisted by the above named, and in the presence of many other medical gentlemen.

Having laid the arm supine, and extended on a table of convenient height, I made an incision about two inches and a quarter long, in the line of the brachial artery, at the lower third of the arm, about one inch and a half above the bend, avoiding the median basilic vein, which lay in my way; cutting through the skin and common integuments down to the brachial aponeurosis, which I next divided on a director, to the extent of the cutaneous wound. I then laid bare the inner edge of the biceps muscle, and with the handle of the knife, worked a little through the cellular tissue, so as to bring the sheath of the vessels fully into view. Having done so, I pinched up a portion of it between the blades of the forceps, and with cautious touches of the scalpel, held horizontally, cut it close to the point of the blades; this made a small aperture, through which I attempted to pass the aneurism needle, armed with a ligature, round the artery; but without success. Meeting with considerable resistance at the radial side of the vessel, I hesitated to make a large opening into the sheath, or disturb the surrounding parts; however, at the suggestion of my medical friends, I enlarged the opening, upwards and downwards, to the extent of about three-fourths of an inch, and when I got a full view of the vessels, the artery, which looked like

a nerve, but redder, seemed not larger than a common director, immediately ceased to pulsate, as likewise did the tumour and radial artery; the inner *vena comes* appeared very large and distended with blood. The cessation of pulsation in, together with the diminished size of, the brachial artery, tended to embarrass me for the moment; but after waiting a little, the pulsation returned, though feeble, and I was then able to identify the artery, and pass a single silk ligature, waxed, round it, without disturbing it to any extent, or at all interfering with the inner *vena comes*, or median nerve. The artery having been tied, all pulsation ceased in the tumour, and radial artery; nor did the patient complain of any pain during the operation, or on tightening the ligature, one end of which was cut close, and the edges of the wound brought together by adhesive strips. A point of suture was made in the centre, and the ligature left hanging out in a line with the noose on the artery. The diminution in size of the aneurismal enlargement was now evident, with a bluish colour of the forearm and hand, and the natural heat somewhat diminished. A compress was laid over the tumour; and a flannel roller put on loosely from the tops of the fingers to the axilla, so as not to impede the enlargement of the anastomosing branches, but to impart sufficient heat to the limb; and the patient was conveyed to bed.

Seven o'clock in the evening, (four hours after the operation.) The forearm and hand are feeling warmer; complains of slight pain in the tumour, and also in the arm; he describes it as if "in the bone;" is inclined to rest; and in all other respects going on very well.

Temperature of the affected limb in axilla 98; bend of arm 92; palm 90. Temperature of the sound limb, in axilla 96; bend of arm 97; palm 98. Pulse at wrist of left arm 84.

℞ Acetatis Morphiæ gr. ¼.
Aceti Distil. ʒj. Solve, et adde.
Aquæ Cinnam. ʒv.
Vini Antimon. gts. xl.
Syr. Simplicis ʒij.
Fiat Haustus Anodynus, hora somni sumendus.

28th. Spent a good night; slept well after taking the draught; pulse 80; no stool; tongue clean. Temperature at axilla 99; bend 96; palm 92. Temperature of left arm at axilla 99; bend 96; palm 96.

Repr. Haustus Anodynus hora somni.

29th. Had a good night, slept well; pulse 80; tongue white; bowels still confined. Temperature at axilla 94 bend 89; palm 92.

℞ Submur. Hydrarg. gr. vi.
Pulv. Jalapæ ℈i.
P. Zingiber. gr. iv.
Fiat bolus, statim sumendus, et habeat
Infusi Sennæ Compos. ℥viii.
Sulp. Magnesiæ.
Tr. Sennæ Compositæ, aa. ʒi. Solve.
Sumat Cyathum Vinosum 3 qr. hora ad solutionem alvi plenam.
Repr. Haustus Anodynus hora somni.

30th. In consequence of the medicines not having operated on his bowels until six o'clock this morning, he did not take the anodyne, but had some sleep in the beginning of the night. Five stools; pulse 80; tongue cleaner; no pain in the incision or tumour. Temperature at axilla 93; bend 92; palm 93; in the palm, under bandage 94. No heat of skin.

Repr. haustus Anodynus.

31st. Had a good night, but complains of pain in the wound to-day; opened the arm, and found the edges of the

incision adhering at both ends, and looking healthy, an obscure pulsation could be felt at the right side, as if a small vessel passed over the tumour, which is diminished in size; pulse 80; bowels regular; tongue clean; temperature at axilla 96; at palm 92; applied fresh adhesive strips and roller.

Repr. Haustus.

September 1st. Slept well last night without the draught; tongue white; pulse 72; bowels confined; temperature as yesterday.

Repr. Medicamenta Cathartica et Haustus; si opus sit.

2nd. Had two stools, and afterwards, at night, took the anodyne; slept well; tongue rather foul; pulse 72; temperature as yesterday.

Repr. Haustus Anodynus.

3rd. On removing the flannel roller off the limb, a tolerably strong pulsation was discernible in the radial artery at the wrist. The suture, which appeared to produce some irritation in the neighbourhood of the ligature, was removed; the edges of the wound uniting for the greater part; two stools; tongue clean; pulse 72; temperature at axilla 95; at palm 92; tumour diminishing; wound dressed with fresh adhesive strips; compress and bandage as usual; to have fowl broth and toast.

4th. Every thing going on well; pulsation at the wrist more perceptible; requires no medicine.

5th. Tongue whitish; bowels confined; in other respects the same as yesterday; temperature at axilla 96; at palm 91; broth stopped.

Repr. Mistura Cathartica.

6th. Feels better; two stools; pulse 80 in both arms; temperature as yesterday; a sanious discharge, tinged with

blood, from the wound; dressing renewed, allowing an orifice of exit for the matter.

Repr. Haustus.

7th. Going on very well; pulse 72; bowels regular.

8th. Arm opened; discharge purulent and healthy; tumour subsiding fast; dressing renewed; bowels confined.

Repr. Mistura Cathartica.

10th. On inspecting the arm, I perceived a lengthening of the ligature, and having pulled it gently, found a resistance; wound healed, except where the ligature hangs out; simple dressing; compress and bandage as usual, with light nourishment.

12th. On examination of the wound, and taking hold of the ligature, it came off without any force this day, being the sixteenth since I tied the artery; the tumour is almost gone, and the patient in the highest spirits. From this forward every thing got on rapidly; some fungoid granulations only shewed themselves in the part occupied by the ligature, which required to be occasionally touched with the nitrate of silver.

On the 3rd of October the wound was perfectly healed, and there remains no trace of the aneurismal tumour, save a little induration of the surrounding cellular tissue; the pulsation of the radial artery of the affected arm continues feeble, but equal to the other in celerity. I saw him on the 27th of October, and found that on the previous day he had been ploughing, and felt as if nothing had ever happened to him.

Although the foregoing case exhibits no great novelty in the morbid appearances, or mode of treatment now generally adopted, for the cure of circumscribed aneurism at the bend of the arm; nevertheless, deeming that its publication may be useful to medical science, I have recorded its details

with I trust sufficient brevity, and at the same time minuteness, as an additional contribution to the knowledge in progress of accumulation upon the subject. And here I would beg to point attention to the fact, that during the time the ligature was on the artery, I took the precaution of keeping a tourniquet on the limb; not that I much approve of its use in these cases, but as a guard against the meddling of the patient, or his friends; a practice too common out of hospital. I have, in more than this instance, observed the remarkable influence exercised by atmospheric air in diminishing the size and pulsation of the large, as well as of the small arteries; which should be borne in mind where these vessels are closely accompanied by large nerves. It would appear also, that the accompanying veins of large arteries in the living subject often bear the same proportion, in size, to the arteries, that the latter do, in the injected state, to the former in the dead subject; the inner *vena comes*, in this case, being as large as the external jugular of a child in croup, whilst the artery was of the diminished size already mentioned. There was no varicose enlargement, or apparent connexion, between the wounded vein and the aneurismal sac. It will be also seen, that the temperature in the axilla of the affected arm became increased for the first few days, owing to the unusual quantity of blood suddenly thrown on the inosculating branches. Dr. Harrison, speaking of circumscribed aneurism from puncture of the brachial artery, at the bend of the arm, says, " I have known several cases of this species of aneurism, and from the same cause, in young persons, in whom a perfect recovery was accomplished by the application of gentle pressure on the part; by bandaging the fingers, hand, and forearm; by rest, and suitable constitutional treatment: I should, therefore, recommend in almost every recent case of this disease, a trial of this practice before having recourse to an operation; at the same time,

however, it is right to observe, that pressure ought not to be indiscriminately applied, nor too long persisted in, as in some cases it may induce absorption, or ulceration in the integuments, and expedite the progress of the tumour to the surface; and in others it may convert a circumscribed into a diffused aneurism, and thus aggravate the disease." Mr. Hodgson, on the other hand, says, "compression has been strongly recommended in the treatment of circumscribed aneurisms arising from wounded arteries; and numerous instances are recorded, in which it is said to have been advantageously employed. The records of surgery abound in descriptions of instruments invented for the purpose of compressing this kind of aneurism, with the intention of preventing the passage of the blood through the wound in the artery. A perusal of many of those cases in which compression is said to have effected the cure of this disease, has, however, convinced me that this mode of treatment cannot be relied upon with a greater prospect of success for the cure of aneurisms arising from wounded arteries, than when the disease is the consequence of a morbid condition of the coats of the vessel; and from the observations contained in a former part of this treatise, I think it is evident that compression is rarely capable of effecting the cure of this disease." All I can say is, that having tried both methods, I am convinced had I persevered in the use of pressure much longer, ulceration of the sac would have been the result, without any chance of producing union of the coats of the artery.

Art. XXVII.—*Observations on Cerebral and Spinal Apoplexy, Paralysis, and Convulsions of New-born Infants.* By Evory Kennedy, M. D., Master of the Dublin Lying-in Hospital.

APOPLEXY OF NEW-BORN INFANTS.

Since the attention of the profession was called to the subject by Stahl and Bichat, and the matter investigated by Ranque and Halle, the predominance of cerebro-spinal susceptibility in the new-born infant has been fully established. Much still remains to be done in elucidating the morbid states and influences of the cerebro-spinal organs. Whether we consider them in regard to the frequency of their occurrence, or their fatality, no class of diseases demands our attention more than the apoplexy, paralysis, and convulsions of new born-infants. The difficulty attending the investigation of these affections, and tracing their connexion with the cerebro-spinal system in the adult, is but too well known to the practical physician; how much is this increased when we come to examine the being incapable of communicating its symptoms? It is quite in our recollection its having been asserted that infantile diseases are more easily detected and diagnosed, from their being arrived at merely through the natural expression of pain, and the sensible features of disease, unaltered and unimpressed by the peculiar fancies or warpings of the rational individual; but this appears as a very questionable proposition. It is certainly a fact, that a few great and palpable features of disease are naturally expressed by the infant; but, when we come to search out nice shades of distinction, and to discriminate between types bearing strong resemblances, we shall find often a great want in the absence of reason, and the power of communicating ideas; a want that cannot be supplied

without so close, discriminating, protracted, and frequently-repeated observation on the part of the physician, that few men possess either patience or sufficiently extensive opportunity to be competent to supply it. Whilst we admit that there are many circumstances productive of difficulty in investigating infantile diseases, some of these even beyond our control, we cannot conceal from ourselves the fact, that medical practitioners do not avail themselves of the most effectual means of overcoming these, by devoting that strict attention to symptomatic and pathological investigation which infantile diseases so preeminently demand.

The most elaborate and accurate inquiries into cerebro-spinal pathology at the present day, have arrived at little more than the threshold of this subject, as far as establishing a fixed relation between diagnosis and disease.

A more accurate knowledge can only be obtained by repeated and candid relation of what has been absolutely observed. It were much to be wished, that those who treat on subjects of this obscure nature, rested satisfied with affording facts, and avoiding embarrassing the mind of the inquirer by putting forward crude hypotheses, however ingenious, which but too often, like the "ignis fatuus," seduce him from the straight paths of scientific investigation. A conviction of the necessity of attending as closely as possible to this rule, induces us to do little more than afford a detail of actual observations made upon cases of these obscure diseases falling under our notice ; in the hope that they may not prove totally devoid of interest or utility to the practitioner anxious to extend his investigations upon this particular class of affections in infancy.

The causes of the predominance of apoplexy in the new-born infant may be ascribed, first, to the so suddenly altered circulation, by which the influence of the maternal circulating system upon the fœtus is done away with, whilst the nice adjustment necessary in the permanent

establishment of respiration an independent circulation, and the right performance of the cerebro-spinal functions, but more especially of their mutual relations and dependencies, is still scarcely completed. The balance that time afterwards fixes so accurately between these important vital organs, as essential to the very existence of the individual, is still unsettled; and, from this circumstance, very slight causes will produce excessive derangement. The lungs, also, at this period are expanded but in part, and a very important change is effected in the circulation of the blood in the heart and great vessels by the closure of the foramen ovale, and the current passing from the ductus arteriosus. From these several causes, then, the blood is frequently thrown in excess upon the brain and lungs, or retarded in its transmission through them. The coats of the arteries are thin and yielding, and, from deranged vital energy in the still imperfectly established functions of the ganglionic system supplying them, the due transmission of the blood may be further interfered with. These considerations, with a recollection of the structure of the brain in the fœtus, which is so soft and vascular, of the compression in the dependent position to which it is liable in birth; and the difficulty and delay often attendant on the first establishment of respiration, will tend rather to excite our astonishment, that cerebral affections in the infant should not be more numerous than they are.

CEREBRAL APOPLEXY OF NEW-BORN INFANTS.

The apoplectic attack in new-born infants may be a simple primary affection, exhibiting, as we see in the following case, all the symptoms of an apoplectic seizure, as observed in the adult.

CASE I.—*Primary or Simple Apoplexy.*

October 14th.—The child of C. W. enjoyed perfect health up to the sixth day from birth, when it refused the

breast, and suddenly fell into a state of stupor, with laboured stertorous breathing. The pulse 60; face tumid and livid; bowels, &c. perfectly natural. One leech was immediately applied over the fontanelle, and another at the lower part of occiput; the spine was rubbed with volatie liniment, and the body immersed in a warm bath, whilst the head was kept cool. The respiration became natural, and pulse rose to 120; shortly after the leeches fell off.

15th. Much better, in every respect; its sensibilities are sufficiently acute; but still cannot suck, not grasping or holding the nipple when introduced into the mouth. To be supplied with breast milk and a small quantity of wine whey through the day. This child continued improving, and was quite well on the 16th.

This case may be looked on in every respect as one of simple apoplexy; and it may be fairly inferred, that the train of symptoms depended upon mere congestion.

Apoplexy may be combined with inordinate or spastic action of the muscles of the face, trunk, or extremities, more or less complete or partial, a striking instance of which we have in the following case.

Case II.—*Apoplexy combined with Tonic Spasm.*

This child exhibited a tumour on the occiput, the effect of a twenty-nine hours' pressure in labour. A few hours after its birth it became insensible, its respiration laboured, and the muscles of the neck and lower extremities were spastically fixed, producing complete opisthotonos. The mæconium had been freely evacuated by castor oil; a leech was applied to the fontanelle, and two to the spine, and the child placed in the warm bath; calomel exhibited in small and repeated doses, the spine rubbed with volatile liniment, and a turpentine injection administered. The sensibility gradually returned, the spasm subsided, and the child recovered.

The cerebral symptoms in the child are, in a large proportion of cases, merely secondary, and symptomatic of disease, or obstructed function in remote organs, as the annexed cases will illustrate.

CASE III.—*Secondary Apoplexy.*

The child of B. C., a boy, had been suffering from its birth under biliary derangement, attended with yellow discoloration of skin, dark, unhealthy evacuations, and abdominal uneasiness, particularly in region of liver, for which he had been treated with mercury, purgatives, and turpentine enemata, the bowels acting freely. On the fifth day he fell into a state of stupor, attended with slow laboured respiration and suffused countenance; pulse eighty; pupils contracted and insensible. After lying in this state for about six hours, a fit of general convulsions ensued, when a leech was applied to the back of the head, and the spine and abdomen were rubbed with stimulating liniment; under this treatment he amended immediately, and left the hospital perfectly well on the eighth day.

This appeared to be a case of secondary cerebral disease, the apoplexy being consequent upon the biliary derangement. It was not, however, the less serious in its nature, from this circumstance, and required the same treatment to remove it as if it had been the original morbid state.

CASE IV.—*Apoplexy from obstructed Respiration.*

A male child, seven days old, was found lying on his face, in which position he had been for some time, his mother thinking he was asleep. When taken up, he was in a state of stupor, countenance livid, respiration irregular; each inspiration was performed with a convulsive motion, the expiration was tar y and prolonged; heart beating slowly and faintly. Frictions, stimulating applications, ammonia, &c. were had recourse to, and after a short time respiration became gradually more frequent and regular, though still

convulsive. A fœtid enema with turpentine was thrown up. He now seemed much improved, the countenance becoming less livid, and he cried frequently. In an hour after, the respiration was observed again irregular and laboured; a similar mode of treatment was adopted, but not with the same success; dilute wine whey was administered by the mouth and rectum, with but partial good effect, as he never completely rallied, and he died in six hours.

Post Mortem Examination, sixteen Hours after Death.—Body stiffened, lips livid, there was not much blood in the vessels or scalp, but on raising the bones a considerable quantity escaped. The veins on the surface of the brain were turgid, there was some transparent subarachnoid effusion. On making a section of the brain, a marked oozing of dark blood was observed on the incised surface, increased by making pressure. There was about half an ounce of fluid in the ventricles. The cerebellum presented a similarly congested appearance. The veins of the spinal marrow were also turgid. The lungs did not crepitate freely under the finger, and when cut into a quantity of black blood was poured out. The larynx was filled with reddish mucus. There was a small quantity of serum in the pericardium. The right side of the heart was filled with blood in a coagulated state; abdomen healthy.

CASE V.—*Apoplexy from Interference with the Functions of the Thoracic Viscera.*

The child of M. F. was large but weakly on birth, when it was immersed in a warm bath and the respiration established. In the course of four hours, however, it fell into a state of stupor, at first apparently sleeping, but at length it could not be roused; the surface of the body and nails becoming blue. The heart's action was not to be detected at the left side, but was perceptible at the right. It gradually became comatose, in which state it continued for some hours, and expired.

Post Mortem Examination.—On opening the thorax, the entire of the abdominal viscera, except the liver and right kidney, were found in the left thoracic cavity, having passed through an enlarged œsophageal opening. The heart and lungs were contained in the right thoracic cavity. The vessels of the brain and membranes were generally much congested, and there was a considerable quantity of serous fluid effused between the arachnoid and pia mater.

Even in those cases where the symptoms present leave us no reason to doubt that the cerebro-spinal system is markedly engaged, we rarely find lesions of the same nature, or to the same extent, in the new-born infant as in the adult, who exhibits a corresponding train of symptoms. Thus we have never met with blood effused into the corpus striatum or optic thalamis in the apoplexy of new-born infants. Where extravasation does occur, it is generally at the base or surface of the brain, and proves immediately fatal. In the majority even of fatal cases of this disease, the morbid appearances observed are merely turgescence of the veins and sinuses, with sanguineous oozing from the structure of the organ itself, and perhaps serous effusion underneath the arachnoid, at the base of the skull, in the ventricles or spinal cord. Little can be said on the treatment of these cases, further than that the success attending them will be found to depend upon combating the cerebral symptoms promptly, whether they constitute the original disease, or merely occur as consequent upon other morbid states.

Depletion to the extent practised would by some be objected to, whilst others would esteem it as not carried far enough to afford the necessary relief. This discrepancy to us appears (problematic as it may seem) to establish the accuracy of the plan adopted. It cannot be doubted that the objection of many practitioners to depletion, in these and similar cases, has arisen from its having been carried

too far,* and been used too indiscriminately. A new-born infant will bear the application of one or two leeches with the happiest effect, in cases of congestion or inflammation, when three or four would prove fatal from the debility induced. In some cases the application of two leeches or even of one, is attended with palor of the countenance, quick pulse, and exhaustion almost approaching to syncope, and requiring the administration of stimulants, as wine whey, to restore the natural tone and energy of the vital powers.

Leeching is both the safest, easiest, and most effectual means of abstracting blood in new-born infants, after it becomes impossible to obtain it from the funis; and whatever some persons may assert to the reverse, experience has quite satisfied us, that without its assistance in the cases above treated of, the fatality would have been much increased. Case No. IV. is one in proof of this assertion; here depletion was withheld, and other means, as stimulants, &c., relied upon, under the impression that too much debility existed to admit of it, and yet this debility, as the result proved, was the effect of the congestion which would have been removed by depletion. In similar cases we have depleted, and followed the depletion with the administration of stimulants with the most marked benefit, nay, it has even been necessary in some to alternate the depletion and stimulation again and again, before the oppression and fictitious debility were removed, and the proper balance between the vital organs established.

SPINAL APOPLEXY OF NEW-BORN INFANTS.

Spinal diseases, the obscurity attending the diagnosis and investigation of which is so proverbial, have of late years attracted much attention. It is to be hoped that this impetus will be productive of satisfactory results in their

* See North on Convulsions, and Davis's Obstetric Medicine.

elucidation, an attainment only to be arrived at by those who possess an opportunity of observing and tracing these cases, recording them accurately. We shall now give a few cases of these affections falling under our notice in the new-born infant; one of the most interesting of them is spinal apoplexy. That dependent on extravasation of blood into the spinal canal is a rare disease; several such, the result of injury, are on record; but its occurrence is not confined to these.*

Dr. Abercrombie records an interesting instance of this lesion, in which the blood was effused into the canal without the theca, in an infant six days old. The most striking symptoms in this case were the fixed state of the jaw, and difficulty of deglutition. Only one case has occurred to us in which this lesion was observable on dissection.

Case VI.—This was one of trismus nascentium, and ran the usual course of that malady, proving fatal in about thirty hours from the commencement of the attack.

The following were the Post Mortem Appearances.—In the spinal canal, without the theca, a considerable quantity of blood was effused; and the veins along the medulla spinalis were very turgid, and filled with dark blood.

A remarkable coincidence then is observable between the case recorded by Dr. Abercrombie, and the one here reported, not merely in the morbid appearances observed, but in the symptoms occurring during life; indeed, so much so, that were we not aware of the extreme accuracy of observation with which that acute and talented physician and author is endowed, we might, upon the reading, have pronounced his case as one of trismus nascentium. On this subject, however, we shall not at present dwell, as trismus

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* See Cases reported by Drs. Bright, Chevalier, and Olivier.

must demand from us a distinct consideration, in recording at some future period the results of certain treatment at present under investigation in this hitherto fatal malady.

As is the case in cerebral, so in spinal apoplexy the train of symptoms strictly apoplectic may occur without actual lesion of vessels, or effusion of blood. This may, with every propriety, be termed congestive apoplexy of the spine. The following are cases of this form of disease.

CASE VII.—*Congestive Apoplexy of Spine.*

On the fifth day from birth, this child was suddenly attacked with screaming, which continued for an hour; it afterwards fell into a sound and protracted sleep, bordering on stupor; and, on awakening the next morning, its face was livid, eyes shut, mouth drawn down at each side, and frothing; and its arms were fixed firmly against its sides. In the course of the morning the breathing became hurried; and abdomen very tense; and it died within twenty hours from the commencement of the attack.

Dissection a few Hours afterwards.—The brain, abdomen, and thoracic viscera, perfectly healthy, but a remarkable vascularity and turgescence of the spinal and medullary vessels was observable throughout their course.

This case would be described as the plethora spinalis* of continental writers, and affords a good specimen of this disease devoid of all other morbid complication.

CASE VIII.—*Congestive Apoplexy of Spine.*

The child of C. G., a healthy boy, was attacked on the third day from birth with a general convulsive paroxysm, after which the arms remained fixed, and face livid. The back of the neck was leeched; and several dark evacuations were procured by aperient medicine. However it gradually

* See Esquirol Bul de la Faculte de Med. Portal Cours d'Anat. Med. tome iii. Frank. de Verteb.

sunk, and expired within twenty-eight hours from the commencement of its illness.

Dissection.—The cellular membrane was found generally very vascular; and the intestines spasmodically constricted in several places. The brain was very much congested, and the vessels of the medulla spinalis, the thecal vessels, and those at the origins of the spinal nerves, were extremely distended and turgid.

In the last case the disease was complicated with derangement of the child's bowels, and the intestines exhibited spastic constrictions, in several parts of their course; these spasms of the intestines may, however, have been referrible to the same irritative or morbid state of the spinal marrow, that produced the inordinate or convulsive action of the voluntary muscles.

The congested state of the spinal vessels, or inflammatory action in the membranes, may, if unchecked, terminate by effusion into the canal, and this effusion will produce such interruption in the functions of the medullary mass and its nerves, as may prove incompatible with the life of the individual.

The following case is illustrative of this fact.

CASE IX.—*Effusion into the Spinal Canal.*

The child of A. E. (a girl) had experienced difficulty in the establishment of its respiration after a protracted birth. The day afterwards it was attacked with general convulsions and violent screaming; the hands remained firmly clenched; the abdominal muscles tense; respiration diaphragmatic, short, and frequent. In despite of leeching and aperients, the convulsions returned, and were repeated at half hour intervals throughout the following day; the child expired comatose in the evening.

The vessels on the hemispheres were much loaded, and the brain, on being divided, exhibited numerous bloody points. A quantity of serous fluid flowed from the sheath

of the medulla spinalis. The medulla oblongata was very firm, whilst the vessels of its membranes, and the venous vessels at the roots of the nerves, were excessively turgid and congested.

Morgagni* gives a case of effusion into the spinal canal, attended with pain and paralysis: it proved suddenly fatal: Chevalier,† one of a child twelve months old, in which paralysis and great pain also were present: it proved fatal in three days. Dr. Abercrombie‡ relates a case in which there was effusion of a gelatinous fluid within the canal, but outside of the theca: in this case, coma appeared to be present from the commencement of the attack.

PARALYSIS OF NEW-BORN INFANTS.

Paralysis in the new-born infant is not a very unfrequent disease; it may occur as the effect of injury to the nerve in the part paralyzed; or in its course, after its transmission through the cranial or spinal aperture. Examples of this we have in injury to the portio dura, as in face presentations; or where the head has been long pressed in the pelvis against the projecting ischiatic spines; several cases of this kind have occurred to us, in which the disease was quite local, the paralysis being removed on the subsidence of the tumefaction produced by the protracted pressure.

To some it may appear that the following case was more than a mere local paralysis of the nerve, yet, on a closer examination, it will be found quite possible that no cerebral or spinal derangement may have existed.

CASE X.—*Case of Paralysis of the Seventh Pair of Nerves.*

G. B. had a tedious labour, the head of the child remaining low in the pelvis for many hours. On birth there was a considerable tumour observed on the scalp, and a

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* Epist. 10, sect. 13. † Med. Chirurg. Journal, vol. iii.

‡ Abercrombie on the Brain, p. 356.

sloughing spot the size of a shilling on the left parietal bone. The delivery took place early on Sunday morning; and about three o'clock on Monday a remarkable alteration was observable in the appearance of the countenance. The angle of mouth was drawn slightly to the right side, while the child remained quiet; on its crying, which it did almost without intermission, the whole face became distorted, the angle of the mouth being then drawn very much backwards and upwards to the right side; the *ala nasi* of the left side not being so much expanded as that of the right, gave the nose a deformed appearance. The eye on the left side remained permanently open, although the one of the opposite side closed in sleeping and crying. The mouth but partly resumed its natural appearance on the child becoming quiet. The brow of the affected side was not corrugated. The child, in other respects healthy, was removed from the hospital in this state.

In most cases, however, the paralysis is markedly combined with cerebral or spinal derangement; in some preceded by a distinct apoplectic seizure; in others it is preceded by a convulsive paroxysm. It often co-exists with convulsions, which generally occur in the side opposite to that paralyzed, as in the following.

CASE XI.—*Paralysis of Portio Dura of right Side, and third Nerve of left.*

June 15th. This child, when expelled, was very feeble; the mother had been fifteen hours in labour; after some time, by stimulation, &c., a convulsive respiration was established. There were two scars noticed, one on the left parietal, the other on the anterior portion of the left temporal bone. This latter was deep, and seemed as if it had been inflicted by some sharp instrument; lower down on the cheek there were one or two scratches; on the opposite side a depression was evident, but without any injury to the surface; the respiration throughout the day was laboured,

with occasional sighing; the left side of the face became somewhat tumid; the right eye was kept constantly open, the pupil dilated and insensible to light; the left eye was kept closed, the pupil also dilated and insensible; an aperient was given, followed by healthy evacuations; some wine whey was administered.

At 5 P. M. The muscles of the left side of the face and body were thrown into convulsions; those of the arm and head were remarkably affected, while the whole of the right side maintained a state of perfect quietude. A bath was administered, followed by an enema.

16th. The infant has remained in a state of almost coma ever since; the left side of the face is more swollen; left eye still closed, the right remaining open; had a number of convulsive paroxysms, brought on by touching or in any way disturbing him, not very violent; experiences some difficulty in swallowing; bowels regular.

17th. Much the same; heart's action 120, laboured; respiration 40. A leech to the neck; wine whey.

18th. Improved by the leeching; towards evening opened for the first time his left eye; expression of countenance more natural; left side of face less swollen; drank more frequently; still some tendency to convulsive motions of the left side, but much less active; pulse 120; bowels free; was put to a healthy, free breast, but could not be induced to suck; took wine whey; a leech was applied, after which he slept quietly. On separating the eyelids at 2 P. M., the eyes were observed turned upwards, so that the pupils could not be seen; pulse now 114, small.

19th. Had a slight convulsive paroxysm yesterday evening; expression of countenance more natural; left side and arm still rigid; pupils less upturned.

20th. Spasms of left side occasionally; bowels not free. To have castor oil.

21st. Looks much better; is willing to suck, but seemingly cannot satisfy himself owing to weakness of the muscles. Head has regained its natural shape; still some rigidity of left side.

Left hospital on the 22nd, and was brought back on the 25th, the mother stating that he had a number of twitchings the day before. He now seems to make more use of his right side.

Occasionally, in these cases, convulsive twitchings occur in the paralyzed extremity, the limb, in the intervals, remaining perfectly palsied. In such, however, we generally find that the sensibility in the paralyzed limb is unimpaired, the functions of the motor tract being alone deranged. The following case affords us an example of this form.

CASE XII.—*Paralysis of the Portio Dura, and Spinal Nerves of right Side.*

The head on birth was slightly compressed; the child soon rallied.

Second day. There is paralysis of the right side of the face, as also of the right arm: on being excited, a slight convulsive motion is induced. A leech was applied to the nape of the neck, and a grain of calomel, followed by castor oil, administered.

Third day. Much the same as to the paralysis. Convulsive twitchings not so frequent.

Fourth day. Has been uneasy through the night. Convulsive startings more frequent; paralysis as before. Repeat the leech.

Fifth day. Much relieved; more quiet; is rather pale seems weak. To have wine whey with breast milk.

Thirteenth day. Has been gradually improving. Paralysis of face less apparent, but has not entirely passed away; wrist still pendulous; in other respects quite well.

Dismissed: the mother wishing to go home.

We occasionally meet with a variety in which a more or less complete paralysis of one side will occur, along with a partial paralysis of the other; perhaps merely one nerve evincing any interference with its functions, as in the following instance.

CASE XIII.—*Hemiplegia of left Side, with Ptosis and Paralysis of Portio Dura of right Side.*

Immediately after birth a large, soft tumour was observed on the right side of the head, principally on the vertex, with two or three small excoriations on the left side. The left eye closed; the mouth drawn to left side; and when the child cries, the *ala nasi* and angle of the mouth at the same side are drawn up; the right eye open; right side of the face unaffected during crying. The left side of the body is completely paralyzed; the extremities are of less bulk than those of the right, and are rough to the touch; the muscles very flabby; both pupils are insensible to light.

On the third day after birth, it had three or four slight convulsions confined to the upper half of the body. It was unable to suck, but deglutition did not seem to be affected, as it swallowed with facility. A leech was applied to the vertex, followed by the warm bath; stimulating liniments were rubbed over the spine; the child recovered and was dismissed cured on the eighth day.

Here then what a capricious selection is made as to the nerves engaged: we have the spinal nerves of the left side generally paralyzed, the functions of the portio dura of the left remaining unimpaired, whilst those of the reverse side are interfered with, and this anomalous state increased by the paralysis of the third nerve on the left. Ascribing the paralysis of the portia dura here to injury, inflicted by pressure, after its passage out of the stylo-mastoid foramen, would do little in simplifying the matter, as it may be inferred that the same cause, whether it were congestion (or whatever else) of the brain or spinal marrow, that

produced the paralysis in the spinal and third nerve, also acted upon the portio dura. In this case, as well as in No. 10, we have a good illustration of that form of paralysis of the motor nerves of the face, which has so much occupied the attention of Bell, Magendie, and Mayo, the portio dura of the right side here being alone paralyzed, whilst that of the left remained unimpaired, the reverse of what occurred in No. 10. However we have an additional interest imparted to the case by the paralysis of the levator palpebræ muscle, supplied by the third nerve, as evidenced in the ptosis with which the left eye was affected, whilst the right remained permanently open from the paralyzation of the orbicularis muscle, which is supplied with its motor nerve by the portio dura.

In cases of paralysis, as in apoplexy, we look upon it, that although they may depend on organic derangements, yet actual lesion, such as ruptured vessel, disruption of the intimate texture of the brain, or even serous effusion, is unnecessary, and rarely (much more so than in the adult) met with, as producing this disease. In the cases which we have had an opportunity of examining, we have seldom found more than a congested state of the vessels of the brain and spinal column, of the meninges* and roots of the spinal nerves. If lesions in the texture of the brain occurred in these cases, as they do in the adult, we should have permanent paralysis remaining also in the infant, a circumstance very rarely met with, if we admit the accuracy of the opinion advocated by some, that when disruption of cerebral texture has occurred, there is never reunion by cerebral matter, nor capability of transmitting nervous influence.

CONVULSIONS OF NEW-BORN INFANTS.

The universal liability of infants to convulsions, is but too well known; and although no climate or latitude ap-

* See Cazanvieilh, Archives Gen. for May, 1827.

pears to afford a protection against these attacks, it would seem that certain localities and states of atmosphere predispose to them.

Those forms depending upon atmosphere or climate, such as prevail in the West Indies, or amongst ourselves in crowded hospitals, or ill-ventilated suburban districts, exhibit peculiar characters, stamping them as what we might term specific diseases; like all diseases owing their existence in a great measure to atmospheric causes, assuming peculiar types and characteristics, and proving generally more fatal in their tendencies. Under this head may be classed the epidemic convulsions described as occurring in Paris, by Gaultier Claubry; the epilepsy recorded by Dr. Longe as occurring at Copenhagen, from which nearly thirteen thousand children perished in thirteen years; the *trismus nascentium*, at present so prevalent in the West Indies amongst the children of the negroes, and formerly so fatal in this hospital, (although now comparatively but seldom* met with here,) every sixth† child having been destroyed by it, according to the report of the late Dr. Joseph Clarke, made in 1792.

Dr. John Clarke‡ is of opinion that in every case of convulsions, the brain is at the time organically affected either directly or indirectly; and Brachet§ states, that every case of convulsions, partial or general, must be dependent upon cerebral irritation. These statements are with equal confidence denied by North and others. It is difficult, we

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* See Collins's Midwifery.

† Mr. North, in his allusion to Dr. Clarke's Report, omits to mention that it was from *trismus nascentium* this large proportion of infants was lost. See North on Convulsions, p. 1.

‡ Commentaries on Diseases of Children, p. 90.

§ Brachet sur les Convulsions.

might say impossible, to arrive at an accurate knowledge of the truth of either opinion. This much, however, may be said, that whether the cerebrum or spinal cord be previously engaged or not, when convulsions are present, there is every reason to apprehend their becoming so, and we should direct our treatment with the recollection of this fact strongly impressed upon our mind.

The frequent occurrence of convulsions as symptomatic of disease, and their rarity as an original morbid condition, are facts now universally admitted. It is perhaps more important than any abstract recognition of the nature of the normal state, that a rational and successful practice is generally established in consequence of the admission of this principle. The profession owes much to Mr. North for the clear and lucid manner in which he has placed this subject before them.

The primary cause of paralysis and convulsions may be described as to a certain extent identical; namely, such an interruption of or interference with the functions of either the brain or spinal marrow, as causes a variation from the uniform balance which regulates the antagonizing muscles, and retains them in a state of neutralization.

Whatever be the actual state of disease or lesion in the cerebro-spinal system in these cases, in paralysis there would appear to extend from it a defective stimulation to the muscle or muscles paralyzed; on the contrary, in convulsions this stimulation would appear to be in excess.

This neutrality of the muscles may however be disturbed in two ways; either by the diminished contraction of one muscle or class of muscles, leaving the antagonizing muscles in excess of action from want of neutralization; or, from excess of contraction in one class, rendering the ordinary contractions of their antagonists unavailing in neutralizing their action. This latter is the state generally met

with in convulsions, the former in paralysis. The excess of contraction may again be either permanent, as we see in tetanic fixture of the limbs, trismus, &c.; or it may be temporary, producing, by the alternating excess and diminution of its contractions, those sudden spastic motions called convulsions. Not unfrequently these very different states of paralysis, and permanent spastic or convulsive action, are confounded, from the fact of the limb occasionally remaining fixed and motionless in both; this difficulty in diagnosis is further increased, by the fact of paralysis of one class of muscles in a limb, say the extensors, occurring whilst the natural contractions of the antagonizing muscles, the flexors, fix it, and cause it to exhibit the appearance we would expect in a limb fixed from excess of muscular stimulation.

The following case is illustrative of that form of excess of muscular contraction, in which there is combined with occasional increases of muscular stimulation, a permanent excess, producing a fixed state of certain limbs, which in this instance exhibited all the characters of catalepsy.

CASE XIV.—*Convulsive Action of Muscles of Deglutition followed by State of Catalepsy.*

This child, a girl, small sized, was very weak when born, the labour had been tedious, and she was with difficulty brought round. The cuticle of the hands, feet, and body was observed to peel off in scales. Cerebral oppression; face deep coloured; lips livid; and a heavy smell from person. Ordered wine whey, and a warm bath, castor oil, and an enema if necessary. Next day, 27th, lies heavy and stupid; rarely cries; respiration abdominal, slow, and irregular; each ordinary respiration followed by two half respirations; bowels freed. Leech to the nape; warm bath. Evening:—Leech did not fasten; child cold, restless; surface dark-coloured; occasionally uttered a bleating scream. On offering drink she became convulsed; the spasmodic action

commencing with the diaphragm; the muscles of the pharynx became next engaged; when swallowing was attempted, the whole muscular system became convulsed. The lower jaw was spasmodically elevated and depressed in the attempt to swallow: the mucous membrane of the pharynx of a deep red colour. Two leeches were applied to the sternum, and calomel and chalk administered: a bath.

28th. Leeches bled well; quieter through the night; deglutition more easy, but still attended with a convulsive movement. Mucous membrane not so red: bowels free. Evening:—Twice during the day uttered three or four bleating screams as if in pain, followed by complete opisthotonos. The eyes became fixed; pupils dilated, not contracting under the stimulus of light; arms firmly flexed; hands clenched; respiration momentarily suspended. In this state she remained for a minute, the heart meantime acting violently. A convulsive sob, occurring irregularly, was the first evidence of returning respiration. Pulse 160. Pupils, after the paroxysm, obeyed the light; but the surface continues dark; lies, in the intervals, with the arms and legs firmly flexed; bowels freely moved; discharge offensive; deglutition improved; has been for some hours comparatively at ease: bath; continue powders.

29th. Convulsions returned in the night; swallows freely; seems weaker; respiration and pulse as yesterday. Continue powders.

30th. No decided convulsions since; but lies with extremities in a state of tonic contraction.

September 3rd. Has for the last three days remained in a state of seeming catalepsy; swallowed whatever was given to her; bowels acted regularly; by degrees she aroused and became more lively; the contraction of the extremities was not so continued. In this state her mother insisted on taking her out of hospital.

The following case is an instance of convulsions occur-

ring as consequent upon retention of the meconium, and deranged bowels—one of the most frequent causes of secondary convulsive attacks observed in new-born infants.

CASE XV.—*Convulsions symptomatic of retained Meconium, and Deranged Bowels.*

This child, a boy, the day after birth refused the breast, and seemed to suffer abdominal pain. A grain of calomel and some castor oil were administered, which appearing to produce griping, the child was put into a bath. Next day, it was stated that the medicine did not act well; the evacuations were scanty, at first blackish, and latterly like dry moss, with a discharge of a considerable quantity of flatus. Towards evening he showed a tendency to convulsions; the eyelids were firmly closed, on separating them, the pupils were contracted; hands strongly clenched; lips compressed; tongue tremulous; respiration laborious; there was some difficulty of swallowing. A slight rash was observed on the upper part of the thorax; the abdomen was tumid; the mucous membrane of the pharynx high-coloured. Ordered an enema, with a few drops of spirits of turpentine, to be followed by a bath. A leech to be applied to the nape of the neck: some wine whey. Next day, 13th, he was improved; bowels acted freely; discharge green and pasty; urine scanty; rash more general; sucks more willingly; seems still to suffer pain; pupils more natural. Calomel, half a grain twice in the day. Evening:—Paroxysms of pain recurred frequently, with convulsive twitches, increased by the discharge of fæces or urine, which is now more abundant. Had a leech applied to the neck at two P. M., followed by a bath, with relief.

14th. Slightly uneasy through the night; convulsive motions not so frequent; bowels once freed; discharge lighter coloured; funis separated. To have some castor oil.

The further treatment of this disease consisted in small doses of calomel at long intervals, with aperients inter-

posed. The secretions gradually became more healthy, and the tendency to convulsive movements decreased in the same proportion; the rash also disappeared. He was attacked on the 16th with ophthalmia, first of one eye, then of the other; and was dismissed on the 26th, cured.

Tissot ascribes cases of this kind to a spastic constriction of the spincter ani preventing the escape of the meconium.

As in more advanced childhood, convulsive attacks not unfrequently occur precursory to the coming out of the eruption in exanthemata, so in the eruptive diseases of new-born infants, the cerebro-spinal system appears to be affected in a marked manner about the same period. The following cases are exemplary of this.

Case XVI.—*Convulsions preceding Miliary Eruption.*

The child of F. M., a boy, when born was very weakly and discoloured, the extremities became quite blue. The mother was at the time labouring under typhous fever. Warmth was applied, and a little wine whey given. In eight or nine hours he was observed to be seized with convulsive twitches, the arms being occasionally flexed with a sudden spasmodic motion; the fingers firmly bent, the thumbs drawn in to the palm of the hand; the muscles of the face were slightly convulsed; bowels confined. One grain of calomel to be given, followed by castor oil. Next day, 21st, convulsive startings have continued ever since with scarcely any interval; bowels relieved; discharge green. The skin is of a yellow colour, and feels exceedingly rough and harsh; there is a most disagreeable fœtor from this child. A leech to the nape; breast milk.

22nd. Convulsive motions as before, particularly of the arms and hands. An eruption, consisting of a number of minute vesicles, is apparent over the head and chest, and in patches on the face. This commenced by a red patch, which on being examined under a microscope, was found to be

made up of a congregation of very minute vesicles with vascular interspaces; the vesicles gradually enlarging, the vascular interspaces disappeared, and it now presents the appearance of a vesicular miliary eruption. Bowels free; discharge black and green. Heart's action 104; respiration 66. To have one-third of a grain of calomel three times in the day: a bath and wine whey.

23rd. Convulsive twitchings continued through yesterday, but towards evening became less frequent; was pretty quiet all night, and had but one slight convulsive paroxysm this morning. Pulse 120; respiration 36.

24th. Attempted yesterday to suck; was given some breast milk, and lay very quietly dozing before the fire; had slight twitches twice in the night; sucked more freely; evacuations more healthy. Eruption has disappeared; skin still rough and harsh; yellow tinge remains.

27th. The cuticle of those places which had been the seat of the eruption is now desquamating. No return of convulsive movements; bowels acting healthily; is lively and sucks well.

CASE XVII.—*Partial Convulsions symptomatic of Strophulous Eruption.*

The child of M. R., a healthy boy, was attacked on the sixth night after its birth with screaming and convulsions, which at first commenced in one arm, and then extended over the whole of one side of the body; recurring at intervals of an hour for several fits.

In the course of a few hours, a profuse strophulous eruption appeared all over the body, forming large blotches over the joints. After the eruption had come freely out, the convulsive fits diminished in frequency and duration, and soon ceased entirely. The child recovered.

In the progress of inflammatory attacks of the viscera or cavities, convulsions occasionally occur, either as sympto-

matic of the serious state of disease under which the child labours, or as the result of the extension of the disease to the cerebrum, spinal cord, or their coverings. The following case exhibits an instance of the latter form of convulsion, and appears to depend upon general inflammatory action in the serous tissues, extending to the serous membrane of the brain and medulla oblongata.

CASE XVIII.—*Convulsions, with serous Effusion into Cranium and Spinal Canal.*

The child of A. L., a girl, was attacked with laboured, panting inspiration on the 26th July, when twenty-four hours' old. Fæcal discharges still dark, treated with leeching, calomel, and aperients, but without relief. On the 27th it had several convulsive twitchings of the upper and lower extremities, the bowels acted freely, panting respiration continued. It remained with little change, the convulsive twitchings occurring occasionally, until the morning of the 28th, when it expired.

Post Mortem Examination.—In addition to copious sero-purulent effusion into the cavities of the thorax, and solidification of the lungs, the vessels of the brain were found very turgid, with its substance of a pink colour; a table-spoonful of fluid was found in the ventricles, and a much larger quantity at the base of the brain, and within the theca vertebralis.

We have already seen, in treating of apoplexy and paralysis, that convulsions not unfrequently occur as the effect of these. On the other hand, convulsions occasionally precede an apoplectic or paralytic seizure. They thus either stand in the relation of cause and effect towards each other, or the same state of sympathy or the same derangement of the sentient and motive centre that produced the one form of disease may give rise to the other. In the following case, a well marked apoplectic seizure succeeded to the convulsions, and a retarded action, bordering on paralysis of the respiratory muscles, attended them. This case is further

interesting from showing the necessity that exists for depletion when the head becomes engaged after convulsions; as we feel no hesitation in stating that the chances of this child's recovery were very much lessened by abstaining from this plan of practice.

CASE XIX.—*Convulsions followed by Apoplexy and Death.*

The child of A. B., a male, was in perfect health until the fifth day, when it got a convulsive fit, after which it became of a dark livid hue; pulse fifty, breathing scarcely perceptible; its bowels had been acting naturally. It was put into a warm bath, and spirit of ammonia liniment applied to the chest, when its breathing gradually became perceptible, but slow. It remained for twenty-four hours in a state of stupor, and insensible to stimulants, an interval sometimes of forty seconds occurring between each respiration; ammoniacal injections were thrown into rectum, the strongest stimulating liniments rubbed upon the surface of the body, and the warm bath used, but it expired about thirty hours from the commencement of the attack. Unfortunately no post mortem examination could be obtained.

ART. XXVIII.—*On the Hydriodate of Potash as an Emmenagogue, in a Letter from* DOCTOR PINCHING.

"TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

"Collon Dispensary, September 6th, 1836.

"GENTLEMEN,

"According to your request, I beg leave to furnish you with a few observations on the action of the hydriodate of potash as an emmenagogue; whether in exciting the uterus to healthy action, or as a general tonic, giving strength and vigour to the constitution, is difficult to determine, I shall merely state what I have observed. The

first patient I was induced to try this remedy with, was the wife of a tenant of the Honourable Baron Foster, who had contracted venereal some years ago from her husband. She had been repeatedly salivated with mercury before she came under my notice, which was about two years ago, when there were extensive phagedenic ulcers of the throat and fauces, extending to the palate, back of the pharynx, with disease of the palate, and palatine process of the superior maxillary, and ulcers on the inside of the cheeks; a good deal of chronic periostitis, with a node on the lower end of the right fibula. I tried the plan of treatment recommended, and which I have seen beneficial in the hands of Mr. Carmichael, I mean sarsaparilla infused in lime water, half a pint three times a day, with alterative doses of Plummer's pill; this I found proved ineffectual. The ulcers I touched alternately with nitrate of silver and butter of antimony, which caused them to heal, but only to break out afresh in a few days. I was induced to try the effects of the hydriodate of potash in large doses, as a last resource; and here I should observe, that I was principally induced to do so from a conversation I had with my friend Dr. Byron, of the Navan Infirmary, who had mentioned to me the good effects he had seen produced by this remedy in his hands; I therefore resolved to give it a trial, and prescribed it in the following formula:

℞ Hydriod. Potassæ, ʒi.
Iodinii, gr. ii.
Tinct. Opii Camph. ℥ss.
Mist. Camph. ℥vss. M.
St. ℥i. ter quotidie.

The bowels to be regulated by castor oil as she should find it necessary. After taking the above, she felt much better in every respect, and the first symptom she described was, the good appetite she got since using the mixture. Her rest at night was improved, and the ulcers looked cleaner, which

I still continued to touch with the nitrate of silver. Ordered to continue hydriodate of potash mixture, and in about two months there was no trace of ulcer; periostitis completely removed; node much better, but still tender to the touch, some slight infiltration about right ancle joint, but what struck me more forcibly was, this woman was about forty-five, has had several children, but the catamenia had not appeared for the last four years; however, since she began to use this medicine, they have again appeared, and continue to come on regularly. Her health is altogether established, and she says she feels herself quite young again; she certainly looks better, and she has got into flesh since the time she first commenced taking this medicine.

"The next case I wish to mention is one which is not perhaps so satisfactory on account of my trying other remedies at the same time she was taking the hydriodate. She was a young girl of a chlorotic appearance, about twenty-six years of age, with rather an inactive liver. I ordered the mixture as before described, together with

℞ Pil Al. c. Myrrh. ʒi.
Pil. Hyd. Sulph. Ferri siccat. āā gr. xx. M.
Ft. Pil. xx. Ht. i. ter quotidie.

"After using these medicines for some time, the catamenia, which appeared very irregularly and in small quantity, came on, and continued quite regular.

"I have at present another case under treatment with an abdominal tumour, in a girl about twenty-two, total suppression of the catamenia, which I apprehend had accumulated within the body of the uterus.

"Should any other case present itself, I shall be most happy to communicate it; and perhaps a more extended trial of this medicine might prove a valuable remedy in obstructions of the uterine circulation.

"Believe me, Gentlemen,
"Yours most faithfully,
"Richard Lloyd Pinching."

Art. XXIX.—*Case of Hydrophobia.* By Richard Long, M. D., Physician to the Arthurstown Dispensary and Fever Hospital.

"TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

"Arthurstown, 24th August, 1836.

"Gentlemen,

"In sending the following case of hydrophobia for publication, I regret it is not in my power to offer any remark of practical utility. It is merely adding another history to the melancholy catalogue of the victims to this direful disease.

"This is the sixth case that has fallen either under my direct care or observation; my experience, therefore, of the different stages of its course has been more extensive than that of many, and afforded me an opportunity of witnessing the total inefficiency of any plan of treatment. I have seen the same fatal results follow blood-letting, mercurial frictions, and fumigations, and the internal use of large dose of arsenical solution and nitrate of silver. The 'alisma plantago,' or great water plaintain, it may be remembered was some years ago announced as an infallible cure by the Russian physicians; of this also I had an opportunity of making a full and patient trial, but alas, to no purpose.

"Some time since it was recommended to immerse the sufferer in a warm bath for several hours, keeping the bath all the time at an even temperature, but unfortunately the most prominent feature of the disease, convulsions, precludes the possibility of following this plan. And I am convinced, from what I have seen of the result of even an attempt at placing the patient in a warm bath, that the fatal issue would be accelerated in a ten-fold degree by such a proceeding.

"The Tonquin medicine was formerly highly extolled as a remedy; its composition was, I believe, cinnabar of antimony, opium, and musk. In reference to this, my practice was adopted in the present case; and certainly its progress presents some features decidedly different, not only from those I before witnessed, but also, in many respects, from any I find on record; whether this difference can be fairly attributable to the modifying influence of the remedy, or not, I cannot determine.

"1st. There was but little acceleration of pulse or increase of external heat.

"2nd. The mouth was not parched, but on the contrary the tongue was soft and flabby, and loaded with mucus.

"3rd. The saliva though secreted abundantly, and presenting the usual frothy appearance, was not ejected with spasmodic violence.

"4th. The power of swallowing liquids freely was fully restored for several hours before death; establishing, by the by, a proof that death is not caused by the inability of swallowing fluids towards the support of the body, but by the specific influence of the morbid poison on the system.

"5th. Death did not take place until upwards of *sixty hours* after the first symptom of horror of water; whereas in those I before noted, dissolution occurred *within forty-eight hours.*

"I have the honor to be, Gentlemen,
"Your most obedient Servant,
"Richard Long, M. D."

Mary Flynn, aged 13 years, had been labouring hard at the bog making turf, for several days, and complained one evening on returning home, of acute pains in the hips and loins, shooting with a cramp-like sensation to the lower extremities; these symptoms excited but little notice, being attributed to her previous fatigue. In a few hours those

pains subsided, but were succeeded by a sharp pain at the pit of the stomach shooting quite through to the back, an occasional catching of the breath, and a marked dislike or inability to swallow fluids.

August 17th. At this stage I was requested to visit her, being about ten hours after first indication of a dislike to fluids.

I found the girl dressed in her ordinary clothes, reclining on a bed; she seemed in a state of great excitement, talking incessantly, but in a manner quite relevant to the subject she spoke on. There was nothing in her countenance which would point out any thing being amiss with her, save an extraordinary, indeed an indescribable expression of the eyes; they shone with a glassy lustre, and the left was much suffused with blood. The skin was cool; pulse small and compressible, 84; tongue loaded with a white mucus of a slight purple tinge. She spoke with ardour of the pleasure she would have in drinking quarts of water from such and such a well; yet when it was offered to be procured for her she would put it off by some futile excuse or other.

Had some drink brought to her; and I was then told that she had adopted the plan before I came, of getting some fluid down without seeing it, by sucking it through the hollow of a straw. This plan, however, I rejected, as I began to suspect what the real nature of the case was, and wished therefore to ascertain clearly the effect which the sight and taste of the fluid would produce. I shall never forget the scene; the start of horror and expression of feature were indescribable. The angles of the mouth were drawn back as in trismus; the eye-balls protruded; and the sense of suffocation was as prominent as that shewn by a person in the last struggles against drowning.

It was now apparent that I had to do with a case of genuine hydrophobia; but by the most minute inquiries I was unable to ascertain that the patient had been bitten

by a rabid animal. All I could find out was, that a strange dog, supposed to have been mad, had been killed near the house about six weeks before; and that several animals, horned cattle and pigs, had died rabid in the neighbourhood within the last six months.

I directed blisters to the nape of the neck and throat, and as the Tonquin remedy, composed of cinnabar of antimony and musk, had been formerly looked on as a specific in this direful disease, I ordered pills containing musk, opium, and calomel, each ingredient in two grain doses; also a warm bath if possible.

18th. Strong cerebral excitement, tending to delirium, continues; horror of fluids as before; had one convulsive fit, requiring much aid to hold her, which lasted nearly half an hour, and was brought on by forcing a spoonful of liquid down the throat; there is now an abundant discharge of white frothy saliva, it is, however, thrown from the mouth without violence; pulse exceedingly small; surface of body cold; both eyes suffused, and swimming in a thick purulent discharge; no discharge from bowels or kidneys, since the commencement of illness; bath has not been used; indeed the girl's friends seem so convinced of the inutility of any means, that I have no great hope of inducing them to do any thing further. She knows all those about her, and talks in most respects quite rationally, bewailing her fate, and the hardship of dying without being sick. Pills to be continued.

19th. Has had several convulsive fits; pulse gone; skin cold; sight apparently lost; muttering delirium; swallows liquids now without difficulty, but is evidently dying.

20th. Continued alive until mid-day, when she died without a struggle.

BIBLIOGRAPHIC NOTICES.

An Essay on the Origin and Nature of Tuberculous and Cancerous Diseases. (Read to the Medical Section of the British Association, on the 23rd of August, 1836.) By Richard Carmichael, M.R.I.A., Corresponding Member of the Royal Academy of Medicine of France, &c. &c. and Consulting Surgeon of the Richmond Surgical Hospital, &c. &c. Dublin, Hodges and Smith.

It gives us pleasure to see our talented countryman, Mr. Carmichael, once more presenting the world with the results of his reasoning and experience: to see him still preserving amidst the arduous duties of his profession the same love of originality which has always distinguished his career, from the period when he produced the first grand improvement in the treatment of syphilitic diseases, in drawing those distinctions by which he has been enabled in a great degree to abolish the abuse of mercury, and to establish an eminently successful practice.

Want of space alone prevents us from indulging in the due meed of praise which the public is justly called upon to give to the individual who prefers seeking out the origin and remote causes of disease, at the expense of time and trouble, which might in a pecuniary sense be much more profitably employed. Mr. Carmichael in this Essay, after a very short prelude, shewing the importance of the study of tuberculous diseases, as one-fourth of the population of these damp climates are subjects of them, rushes, as he says, "in *medias res*." He takes a brief review of the theories eminating from Laennec, Carswell, Bayle, Todd, Clarke, Andral, and others, with most of the arguments adduced on both sides as to tubercle being an organized body, and rejecting the opinions put forward of the softening of tubercle from the centre to the circumference, or *vice versa*, as not of much consequence to the discovery of the true nature of tubercle.

Mr. C. reports experiments of Drs. Jenner and Baron with the same results, and discovers the hydatid or vesicular bodies

delineated by Carswell, thereby proving that a diet of a quality unfitted for due nourishment, when combined with damp, vitiated atmosphere and privation of exercise, are capable of producing either vesicles or tubercles; from these data *alone* Mr. C. objects to the deduction being drawn, as by Dr. Bacon, that the first appearance of tubercle is always vescicular or hydatid. Mr. C. indulges in a well merited sarcasm on the prevailing and general doctrine, that tubercles are caused by that undefined, multiform, or protean state of the constitution termed scrofulous, a term only used to cloak ignorance, and applied to diseases essentially different.

We cannot enter here into all the arguments used by Mr. C. to combat those hitherto adduced on the inorganizability of tubercle, but we must hurry to the theory which he has formed, in his own words:—

"If tubercles then are not depositions of unhealthy lymph, capable of becoming organized by an extension of the vessels of the tissues in which they are placed, what are they? In one word, it is my opinion, that they are beings possessing a vitality independent of the animal in which they are lodged, except so far as that animal affords them; 1st, the organic particles of which they are formed, and 2ndly, the nutriment which they imbibe by their own innate powers; and thus that they form the last link in the chain of the last class of animals, the entozoa."

For the reasoning which leads to this opinion the reader must consult the work, as we wish to give notice of new works rather than to review them.

Twenty-five years ago Mr. C. advocated the doctrine which has been termed that of equivocal or spontaneous generation, in a work upon cancer, and this subject is pursued in the essay before us by illustrations of a very striking character: we make a few extracts.

"A consideration of the primary principles of vegetables, carbon, oxygen, and hydrogen, when compared with those of animals which are the same, with the addition of azote, should induce us *a priori* to suppose that animal matter whenever the vital principle is weakened or nearly extinguished, is more prone to fermentable changes, and consequently to the production of parasitic beings, than vegetables which arise in both kingdoms whenever a diminution of vitality loosens the bonds which tie together the component organic particles of which they are composed, and permits them to assume new shapes of existence. It is thus that the simplest forms of organic life are to be met with, as parasitic plants in the vegetable, and the entozoa in the animal kingdom, where every circumstance seems prepared and favourable for their reception. The mischievous effects of parasitic plants in the vegetable world,

has long engaged the attention of the botanist and agriculturist; and to those of the animal kingdom which are still more general and destructive, I urgently call the attention of the public.

"The analogous disposition to the production of parasites in both vegetables and animals has been long apparent to me, perhaps chiefly in consequence of the vast number of malignant cases which during a long course of public and private practice have come under my observation. The most remarkable instance amongst them which excited my attention to this analogy was that of an old gentleman, whom I saw many years since in consultation with Mr. Macklin. There were few parts of his body to be seen that did not betray the growth of cancerous formations. The forehead, nose, prominent parts of the cheek, lower lip, breasts, testes, &c. exhibited the scirrhous, cartilaginous, or horny masses, which cancer universally exhibits; and such was the general prevalence of the disease, that I could not avoid remarking to Mr. Macklin at the time, that this old man strongly reminded me of an ancient oak overspread and exhausted by fungi."

We must now wind up this imperfect review, by stating the conclusions at which Mr. Carmichael has arrived, in his own words.

"If my views of these diseases be correct, and founded in nature, another, but a lower link will be added to the entozoa, which according to Cuvier, belongs to the zoophytes, (2nd class). The following species may at present be enumerated :—

"1st. Tubercle of the lungs and other parts, whether commencing in the form of a grey semi-transparent vesicle, or of a whitish medullary substance.

"2nd. Masses of tuberculous matter in the abdomen, which either commence in the hydatid form or in that of medullary tubercle.

"3rd. Fungus medullaris, and fungus hæmatodes.

"4th. Carcinoma."

The paper concludes with a postscript containing the objections made against Mr. Carmichael's theory at the Bristol meeting, together with his answers to those objections.

A History of British Quadrupeds. By Thomas Bell, F.R.S., F.L.S., Lecturer on Comparative Anatomy at Guy's Hospital. London: John Van Voorst, 3, Paternoster-row; each number 2*s.* 6*d.*

We have received the first number of this monthly periodical, which reflects the greatest credit on its author, from the manner in which it is gotten up; the purity and elegance of the style; andthe clearness and beauty of the wood engravings.

We are glad to see such a work, one which may be considered of national importance, emanating from the pen of a

gentleman so fully capable of doing justice to a subject of such great interest not only to the medical world, but to the public in general.

This part contains "Cheiropetra;" Bats, genus Vespertilio; family Vespertilionidæ.

A Treatise on Tetanus, being the Essay for which the Jacksonian Prize for the Year 1834, *was awarded by the Royal College of Surgeons in London.* By Thomas Blizard Curling, Assistant Surgeon to the London Hospital, and Lecturer on Morbid Anatomy. London: printed for J. G. and F. Rivington, St. Paul's Church-yard, and S. Highley, 32, Fleet-street. 1836. Price 8*s.*

This treatise contains almost all that is known on this intricate disease, and although not professing to bring forward any novelties in treatment, yet has much to recommend it, from the vast quantity of facts which it contains. It treats of tetanus, in its various forms and varieties, symptoms, termination, statistics, causes, prognosis and diagnosis; pathology, a general summary, containing the theories of the disease, treatment, divided into *local*, as amputation and division of nerves of injured part—and constitutional, as mercury, purgatives, bloodletting, counter-irritation, opium, tobacco, antimony, cold effusion, baths, (warm and vapour) tonics and stimulants, carbonate of iron, hydro-cyanic acid, &c. &c. At the end there is a most useful table affixed of 128 cases of *traumatic tetanus*, which gives, at a single *coup d'œil*, the age, sex, period of injury, symptoms, result, period of treatment, and the remedies employed.

The work concludes with a very extensive bibliography, which is almost a necessary appendage, as very few monographs exist on the subject of tetanus.

The Human Brain, its Configuration, Structure, Development, and Physiology; illustrated by References to the nervous System in the lower Orders of Animals. By Samuel Solly, Lecturer on Anatomy and Physiology in St. Thomas's Hospital, &c. &c.; with twelve plates. London: Longman, Rees, &c. 1836. Price 12*s.* 6*d.*

We agree perfectly with the author of this work, that the system usually hitherto employed in dissection of the brain, by cutting it in slices, is both faulty and unscientific, and leads to little more than the committing to memory of a barren cata-

logue of names; as also, "that the only philosophical method of simplifying and giving a character of general interest to the anatomy of the human brain, is by commencing with the structure and functions of the lowest and simplest forms of animal existence, and carefully observing each addition of parts, and the relationship borne by these to an addition of function." This course the author has pursued in a most interesting comparative anatomy, previous to entering on the anatomy, physiology, and the physiological inferences from pathological states in the brain of man. The principal theories with regard to the cerebro-spinal axis are entered into at length; and the experiments of Flourens, Bouillaud, Rolando, &c. are duly weighed and considered; but it is upon the ganglionic system that we found this author most interesting.

We know not how Mr. Solly has escaped being infected with the *mania phrenologica* of the present age, engaged as his mind evidently has been, in a deep consideration of functions; but we find him even denying conclusions drawn by Gall and Serres: he says, "that erection and emission do not appear essential or pathognomonic symptoms of irritation of the cerebellum;" "that he had never seen paralysis of sensibility, or general sensation, accompanied with complete disorganization of the cerebellum, &c." and in the conclusion, for not having considered the great *hemispherical ganglia* as capable of performing separate offices in correspondence with the different kinds of mental manifestations, his modest apology must fully satisfy the most scrupulous disciple of Gall or Spurzheim: viz. "the whole subject of phrenology appears to me of far too much importance, to be discussed without the most rigid and impartial examination of the immense body of facts adduced in support of it; and this I have not *hitherto* had leisure to undertake."

The plates are delicately and beautifully executed, and highly illustrative of the subject matter.

Manuel de Médicine Operatoire, fondée sur l'Anatomie normale et l'Anatomie pathologique. Par J. F. Malgaigne, Professeur agrégé de la Faculté de Médicine de Paris; Chirurgien du Bureau central des Hopitaux; Membre de la Société Medicale d'Emulation, et de la Société Anatomique de Paris; Correspondent de la Société Academique de Marseille, de la Société des Sciences Mèdicales et Naturelles de Brouxelles, de la Société Medico-Chirurgicale de Berlin, &c. Sécurité, simplicité, celerité. Second edition, considerably corrected and enlarged.

The position which the author of this work has held amongst his scientific countrymen has enabled him, as he states in his

preface, to look down as it were from a height on the progress which operative surgery has been making in his country; and we feel bound to say, that he has fully availed himself of the fine opportunities afforded to him.

The second edition, freed from many of the errors incident to all compilations, which the author had fallen into, from following too closely the beaten track of those who had preceded him, is now presented to the public in a very creditable form; and although professedly intended for the use of students, we think it may, with great propriety, find a place in the library of the lecturer on surgery, and in that of each practitioner who wishes to form an accurate notion of each operation as practised in the Parisian hospitals. This work ranges over a very widely extended field, from the simple excision of a corn, to the removal of the larger joints, and the capital operations which are the boast of surgery; the anatomical descriptions are sufficient without being too diffuse, and the name of the operator, and the peculiar manner pursued by each, is invariably attached to the operation, with the improvements which have been made from the days of Celsus to the present time.

The whole work is divided into two sections; the first containing five chapters, which treat of the general elements of operations. The second, operations (common or general) included under the term "*petite chirurgie*," in ten chapters. Ten more chapters are devoted to operations on the organs of sense, the cavities, genital organs and their appendages, herniæ, those necessary in diseases of the rectum and anus, and those in midwifery and tokology.

In the first section are considered instruments, methods of operation, cautery, caustics, ligatures, means of preventing hæmorrhage, reunion, and the means of alleviating pain during operations.

Second section. Common operations such as venesection, application of leeches, cupping glasses, arteriotomy, blistering, vaccination, acupuncture; all operations on the epidermis; dental surgery; those operations which engage the skin and cellular tissue; restitution of mutilated parts, or autoplastic surgery; operations on the muscles, and their dependencies on the nervous system, on the veins, on the arteries, on the bones, on the articulations, amputations.

In the last ten chapters, operations on the appendages of the eye, on the eye, on the nose, including the rhinoplastic operations; perforation of the frontal and maxillary sinuses; polypi, &c.; on the mouth and its appendages; on the neck; on the thorax; on the abdomen; on the rectum and anus; on the genital organs, (male and female;) and the operations occurring in obstetric practice.

We willingly speak favourably of this little work, yet we are not satisfied with the apology which the author has made with regard to the total omission of luxations, he not considerring the reduction of them as ranked amongst operations perfectly called surgical; we feel sorry at this omission, as the work is in other respects an excellent book of reference.

Die Controverse über die Nerven des Nabelstrangs und seiner gefässe, &c. &c. Von J. A. C. Schott, M. D. Frankfort, 4to, 1836.

The Controversy concerning the Nerves of the Navel String and of its Vessels, &c. By J. A. C. Schott, M. D., Frankfort, 1836.

At first sight it might appear surprising to us to find so many conflicting opinions, and these of such distinguished men, on a matter apparently of such narrow limits as the anatomy of the navel string. One would suppose that half an hour's careful examination with the scalpel would anticipate all future inquiry, and terminate all disputes. This is not the case, however, and it is precisely because of our inadequacy to detect the finer anatomical structure, that we are obliged to "eke out" our information by the assistance of physiological phenomena. Nor is there any thing in the least degree erroneous in the correct application of this principle; but it will often happen that men to whom truth must appear in a sensible form to be recognized as truth—not being able to obtain *demonstration* of a fact, reject all *proof* derived from other sources. Thus with the question before us, i. e. whether the umbilical cord and its vessels possess nerves or not; those who rely upon anatomical research deny their existence *in toto*, and clinch the argument (!) by saying, that of course there are none, *because* there is no use for them. Others allow that they may be seen at the umbilical termination of the cord, and really would be very glad to admit of their presence throughout, especially (Reicke) as physiological evidence is in favour of this view; but, alas! they are "practical men," "matter-of-fact men," and as they cannot find these poor nerves, of course they do not exist. Others there are, who, regarding functional phenomena as facts, as much so certainly as anatomical details, and recollecting how deficient are our means of ascertaining minute structure, believe, that in many cases the former may become the interpreter of the latter, that a functional fulfilled, implies necessarily the agents of its fulfil-

ment; these, we say, have all asserted the existence of these nerves.

Out of these various opinions and their asertion by numerous authors, Dr. Scott has contrived to make a very respectable quarto volume, illustrated by some very nice engravings, and adorned by a portrait of that really great anatomist, Fred. Tiedemann. The book is very neatly "got up," and although there is somewhat of "book making" displayed, still it is an advantage to have so many opinions brought before the reader at once. Besides this, Dr. S. has followed out more fairly than his predecessors, the physiological and pathological arguments which bear upon the question.

The first chapter is devoted to the enumeration of those who deny the presence of nerves in the funis, and to a statement of their reasons. Amongst others we find Galen, Fabricius ab Aqua pendente, Wharton, Needham, Schaeffer, Hoboken, Dimmerbroek, Sir R. Manningham, and Rœderer; these are absolute in their opposition. Haller, Wrisberg, and Lobstein do not quite deny their existence, but not having seen them, they do not believe in their presence. The investigations of Dürr and Riecke were carefully conducted, and had consequently the greater influence, and they declare (contrary to the evidence derived from physiology)that their anatomical researches do not warrant the belief that the vessels are supplied with nerves.

So much for the great names on the negative side, as great, perhaps, as any that can be brought against them; but there is this difference, however, that the authors of modern times, possessing all the improved modes of investigation, and availing themselves of all the additions to our information on the subject of embryology, and on the inter-dependence of organic functions and structural anatomy,—these men are all (or nearly so) in favour of allowing nerves to the cord. On this side are ranged Verheyen, Teichmeyer, Clemens, Trew, Pascoli, Kreuse, Kümpel, Herholdt, &c.

Ribes and Chaussier describe these nerves as being very fine, and ordinarily arising from the hepatic plexus.

Larrey observed them very distinctly in the fœtal calf, originating from the sympathetic.

Osiander concludes from his experiments that the vessels of the cord, like other vessels, are supplied with nerves, which he thinks are ultimately lost in the fœtal portion of the placenta.

Hipp. Cloquet devides the hepatic plexus into an upper and an under portion; from the latter, he says, the placenta and cord are supplied.

Home and Bauer have described nerves supplying, not

merely the arteries of the navel string, but the whole of the substance (fœtal and maternal portions as they were called) of the placenta. These observations have been since confirmed by Dr. Kilian and Professer Weber.

After describing with sufficient minuteness the opinions of his predecessors, the author prepares for the announcement and establishment of his own, by entering upon the consideration of the fœtal circulation in the young of man and some animals, but as he has added nothing essentially new on this point, and as it is very accurately laid down in the different works which treat upon the subject, we shall pass over this second chapter, and proceed to the analysis of the third and fourth, which contain a scrutiny into the anatomical and physiological grounds for denying or admitting the presence of the nerves, and also the evidence of such supply derived from embryonic pathology.

We shall lay before our readers a brief account of these two chapters. But first, as to Dr. Schott's own opinion, it is this; without saying that the nerves are traceable in their subdivisions by the knife of the anatomist, he believes, from a combination of evidence, that the nerves which are visible at the umbilical insertion of the cord, do actually accompany the entire development of the umbilical vessels, even to their termination (in the placenta) in the returning veins.

Now the first objection Dr. S. considers is, that founded upon our being unable to trace them; and in answer to this, he adduces other examples (the vertebral and cerebral arteries) of vessels, upon whose ramifications no nerves can be traced, and yet whose possession of them is not denied.

2. The author contends, that the insensibility of the cord (objected by some writers) results necessarily from the fact of its being supplied by nerves from the ganglionic system, which we know are only sensible to their own proper stimulus during health. In a state of disease, parts so supplied do often acquire sensibility, and the author states this to have repeatedly occurred to the funis.

Having met these two objections, Dr. Schott proceeds to the enumeration of arguments favourable to his view.

1. The irritability of any part (it will be admitted, he says) is in proportion to the nervous power allotted to it. Now of all arteries, those in the umbilical cord appear to be the most irritable, therefore they must have a supply of nerves.

2. Osiander, sen. considered, and our author agrees with him, that the arteries of the cord have in some degree a power of independent action,—an opinion founded upon his observing pulsation in the cord after the heart had ceased to beat. Naegele relates a similar fact. But independent action involves necessarily the presence of nerves.

3. The nutrition of any part must essentially depend upon nervous influence; now the arteries themselves increase with the elongation and augmentation of the cord, and must consequently be thus influenced.

4. Secretion is undoubtedly the result of nervous action; now if, as is believed by Monro and others, the liquor amnii be secreted by the terminations of the umbilical arteries, they must doubtless be well supplied with nerves.

After adducing these arguments at considerable length, Dr. Schott proceeds to strengthen his position by analogy with the vesels of another organ which are confessedly endowed with nerves. He compares both the structure and function of the placenta and cord with that of the lungs and its artery.

Both certainly are employed in transmuting venous into arterialized blood; both are covered by serous membranes; both divided by fissures into lobes and lobules; the blood enters both by arteries and leaves it by veins; both have arteries for their own nutrition, &c., &c. Now if the vessels of the lungs have nerves, even though, according to Bichat, they be very few, surely they should not be denied to the umbilical vessels. Again there is some analogy between the umbilical vein and the vena porta. Their structure and functions are similar; both are supposed to possess a power of independent motion; and the nerves of each are visible only at their large extremity. If therefore we admit that there are nerves accompanying the branches of the vena porta, we must, in justice, do the same for the umbilical vein.

As to the doubts felt of their existence, this does not prove that they will not be discovered. It was doubted if the uterus was supplied with them (according to Dr. Schott) until Tiedemann demonstrated them; and also whether they were present in the dura mater, until they were shewn by Tiedemann, Arnold, and Warrentrapp.

We next come to the evidence afforded by pathology. The author takes it as a principle that nerves are necessary to the production of disease; and as disease is occasionally observed in the cord, placenta, and fœtus, this would seem to imply their presence.

Zandt and Dürrs' cases of diseased placenta are quoted, and Meckel and Sasses, and Osiander's cases of inflammation of the cord. A doubt is also thrown out as to whether several infantile diseases (icterus neonatorum, and abdominal erisipelas) may not commence in the vessels of the cord, and be caused by rough handling after birth.

Again, it must be by the nerves that disease is transmitted from the mother to her child; and cases of intermittent thus communicated are quoted. Impressions on the maternal ima-

gination are also thus transmitted, and are especially effective in deforming the child when received at a very early period of utero-gestation, and illustrations are adduced from Hufeland, Osiander, Schaeffer, Baldy, &c.

The author might as well have adduced the communication of syphilis and small-pox, of which authentic examples are on record. But it must be remembered that there is another way by which disease *may* possibly be transmitted, viz. by the blood. Until it is found that this is not the case, half the weight of Dr. Schott's arguments is necessarily lost.

The fourth and concluding part of Dr. Schott's volume is occupied with a minute investigation of the opinions of his opponents: into this, however, we cannot enter.

In conclusion we may observe that it is in the highest degree probable, from physiological evidence, that these vessels of the cord are endowed with nerves; that analogy supports this view of the matter, and that anatomy, whilst it furnishes but little for, furnishes nothing against this supposition. We do not always approve of Dr. Schott's reasoning, but we agree with his conclusions, and we refer our readers to his work with pleasure. Our analysis, though accurate, is perhaps rather short to do full justice to the labour he has bestowed upon his volume. The plates are very well executed.

F. C.

On the Disease of the Hip-joint: with plain and coloured Plates. By William Coulson, Consulting Surgeon to the London Lying-in-Hospital; late Surgeon to the General Dispensary; Fellow of the Royal Medico-chirurgical Society; Member of the Hunterian Society; and Corresponding Member of the Medico-chirurgical Society of Berlin.

This work, amongst many other reasons, will be found valuable to the pathologist from the descriptions of dissections obtained in the early stage of the disease, and which we do not recollect to have seen in any other work. From the appearances presented by those dissections, the author has been led to form the inference that the disease commences most frequently (if not invariably) in the synovial membrane; and he argues that in many cases, from finding the round ligament early destroyed, that it next suffers, then the cartilage, and finally the bone. The work contains—

1. Anatomy and physiology of the hip-joint.
2. Of the cause of the disease.
3. Pathology of hip-joint.

4. Morbid anatomy of hip-joint in this disease.
5. Symptoms.
6. Diseases with which it may be confounded.
7. Treatment, local and constitutional.

The plates are well executed, and the work, in addition to its utility, is got up in a handsome, nay, we would say, in a fashionable manner.

Guy's Hospital Reports, No. III. September, 1836.

The third number of these reports fully sustains the character of its predecessors, and claims the attentive perusal of every member of the profession. We hope that the example which the physicians and surgeons of Guy's Hospital have given, will be followed by the medical attendants of all the chief hospitals in the kingdom. We think it unnecessary to dwell on the immense practical benefits likely to arise from the publication of the experience acquired in such institutions; and we refer with pleasure to the volume before us as a specimen of the advantage to be derived from the proposed plan. It is truly delightful to be enabled, by the aid of such works, to accompany, as it were, the most accomplished professors of the healing art into their wards and dead rooms, and to witness them employed in investigating the causes of diseases, and the means of alleviating or curing them. The subjoined chapter of contents will put our readers in possession of the subjects treated in the third part of Guy's Hospital Reports.

Some observations on the nature and treatment of ganglion, bunion, &c. &c.; by Mr. Aston Key. (With plates.)

Observations on the thyroid gland, by Mr. T. W. King; with notes on the same subject, by Sir Astley Cooper. (With plates.)

Some experiments and observations on tying the carotid and vertebral arteries, and the pneumo-gastric, phrenic, and sym-sympathetic nerves; by Sir Astley Cooper, Bart.

Observations on fatty degeneration of the liver; by Dr. Addison. (With plate.)

Remarks on cystine, or cystic oxide, and its existence in urinary deposits; by Mr. Goulding Bird.

Case of a large bony tumour in the face completely removed by spontaneous separation. To which are added, observations upon some of the functions of the soft palate and pharynx; by Mr. Hilton. (With plates.)

Cases occurring in the clinical wards.

Cases of hernia, popliteal aneurism, wounded ulnar artery, and malignant tumour; with remarks, by Mr. Bransby Cooper.

Observations on chlorosis, and its complications; with cases; by Dr. Ashwell.

Case of wound of the abdomen, followed by protrusion of a large portion of omentum; with observations, by Mr. Aston Key.

Ophthalmic cases occurring at the Guy's Hospital Eye-Infirmary, under the care of Mr. Morgan, Surgeon to Guy's Hospital, and Mr. Jalland Edenborough Assistant to the Infirmary. (With plate.)

Account of a remarkable misplacement of the stomach; by Dr. Bright. (With plate.)

Observations on jaundice; more particularly on that form of the disease which accompanies the diffused inflammation of the the substance of the liver; by Dr. Bright.

Observations on the situation and structure of malignant diseases of the liver; by Dr. Bright. (With plates.)

Appendix to notes on the thyroid gland; by Sir A. Cooper, Bart.

Appendix to experiments on tying the Arteries and Nerves in the Neck; by Sir Astley Cooper, Bart.

Appendix to Dr. Bright's paper on jaundice; by Mr. Rees.

Illustrations of the museum; by Dr. Hodgkin.

A short Treatise on Medical Reform. By W. B. Wilmot, M.D., Stroud.

Doctor Wilmot's pamphlet is written with a tone of moderation which cannot fail to please, and will insure him the good-will even of those who may not approve of the plan he recommends; and which is as follows:—

"That the members of the medical profession do form amongst themselves reading societies, where also matters relating to the internal policy of the profession may be discussed; and that a chain of communication be kept up throughout the entire body.

"That the large towns be divided into convenient districts, according to the medical population, and that each separate association be represented by one or more delegates at a general meeting of delegates from each separate society, at stated intervals of time, within the said town.

"That in the smaller towns, where the medical population is much thinner, there be similar societies formed, with a convenient place of meeting, where each association, according to their

localities, may concentrate also at stated intervals: and from each let delegates be sent to the county town, or should this be so populous as to form a sufficient circle of its own, to some other town of importance, where each society may be represented by its delegates, in a general meeting of delegates within its prescribed boundary, also at stated intervals.

"That each centre thus formed be in communication with others.

"Lastly, that each central point be represented by one or more delegates, at a general meeting of delegates, from each other central point, annually or triennially, or at any other more suitable specification of time, either in the metropolis, or other convenient centre, as may be agreed upon by the general body.

"Thus may we stand as a profession presiding over the medical interests of society, no longer the ghostly spectre, the mere shadow of power, which is our condition at present, but a real substantial body of perfect symmetry, having a head to dictate, and members to execute the suggestions of reason."

We must confess that this proposal, however plausible, seems to us likely to be found, on trial, incapable of being efficiently worked; and we are of opinion, that any well digested and permanent plan of medical reform can only be expected to emanate from a central board of eminent practitioners, selected by the legislature, and endowed with the power not merely of regulating medical education, but holding in their hands efficacious means of repressing quackery. If the public are to be left at the mercy of every advertiser of nostrums, no legislative enactments can benefit the regular practitioner. The extent to which empiricism is carried in Great Britain, may be proved by the columns of any of our daily papers, which abound in advertisements of pills, balsams, and other boasted specifics, gravely and emphatically recommended as infallible remedies for every disease which flesh is heir to. Let any thinking and rational man carefully peruse a dozen of these advertisements, and he will at once acknowledge that the public must be very easily deceived, if they allow themselves to be imposed on by such barefaced and shallow statements. And yet, such is the fact; human nature, ever credulous, is prone to be attracted and misled by even the most clumsy fabrications, provided that the fabricators draw fearlessly on the credulity of their dupes, and proclaim, without compunction, the infallibility of their nostrums. Until this shameful system is put down by the strong arm of the law, the Profession can gain but little by medical reform: though much remains to be done in improving our medical and surgical institutions, yet more, much more, reform is required beyond the immediate sphere of the profession,

than within it. The flourishing state of quackery is a disgrace to Great Britain, to which all Europe points with scorn and derision.

A Bedside Manual of Physical Diagnosis applied to Diseases of the Lungs, Pleuræ, Heart, Abdominal Viscera, and Uterus. By CHARLES COWAN, M. D.

WE hasten to acknowledge the receipt of this valuable little book. We can say with truth, that it forms a most precious addition to our stock of practical treatises, and will prove of the greatest use in enabling the student to form an accurate diagnosis of some of the most important diseases to which the human frame is liable At the small price of half a crown, we are here presented with a most faithful, pithy, and clear account of the physical signs, which impart an accuracy to our investigations in many cases quite unknown to our predecessors. In perusing its contents, we are forcibly struck with the idea, how much Cullen, Gregory, and Bailley would have been surprised had they had an opportunity of studying it after they had written their celebrated works on diagnosis. Here they would find an entirely new, and as far as it applies, a nearly certain mode of explaining the nature of certain maladies laid before them—a mode which they would have prized beyond all price. We who practised before the introduction of physical diagnostics into medicine, are most thankful for the improvement, and that we have lived to witness, not its mere commencement, but its completion. Dr. Cowan was well fitted for the task he has undertaken, and has performed it in a manner which leaves nothing to desire. He has succeeded in attaining brevity without sacrificing perspicuity. None but those conversant with the subject matter of his researches, can appreciate the extent of reading and the accuracy of observation necessary for the execution of this Bedside Manual. We rejoice at seeing full justice rendered by Dr. Cowan to our numerous fellow-citizens who have so eminently distinguished themselves in improving physical diagnosis; and at observing that Dr. Cowan acknowledges the value of many original contributions printed at various times in this Journal.

SCIENTIFIC INTELLIGENCE.

The translation of the following interesting article is from the pen of Mr. Goetz, a gentleman whom we can most strongly recommend as a teacher of German to Medical Students. His address may be learned by applying to our publishers, Messrs. Hodges and Smith.

Some Remarks on the Sensations of the Amputated, by Dr. G. Valentin, Breslaw.—Although it has been long known already even to unprofessional persons, that those who have undergone amputation usually retain the sensation of their lost limb, still less attention has been hitherto devoted than it merits to a subject equally important in a physiological and pathological point of view. As it is the case with so many subjects of practical medicine, an attempt was made also here to explain the matter before it was accurately known what required explanation. Thus Lemas, in his Dissertatio, quæ Dolorem Membri amputati remanentem explicat, Halæ, 1798, 8, which appeared under the presidency of Ph. Fr. Meckel, labours through thirty-two pages with the minute representation of his hypothesis of explanation, whilst at the conclusion, in the form of an appendix, he quotes merely two examples, both of which, in regard of the subject to be proved, are unsatisfactory, in so far as their observation was made too short a time after the operation. Besides the cases which John Müller has especially mentioned, (Physiology, I. 675), only the facts that were already more known, were chiefly illustrated ; that the amputated retain for their whole life the deceptive feel, as if they still possessed the lost limb in complete integrity, except that the latter appeared to them to be benumbed as soon as all the branches of the nerves of the stump were pressed. Many other symptoms, manifestly constant and worthy of more accurate consideration, are, however, either mentioned even here only *en passant*, and occasionally, and many equally important ones, on the other hand, are not at all alluded to. As I had directed my attention already, in the years of my studies, to this paradoxical phenomenon, and as I have endeavoured to pursue the subject as distinctly as possibly on every occasion presented to me, I think I can communicate here some not entirely superfluous remarks, regarding facts, the definition of which I leave voluntarily to the ingenuity of others,

though they may contain less of what is new than what is necessary to confirm and to limit.

My observations have been instituted on from twenty to twenty-five persons who have suffered amputation in the most different parts, and have been very frequently repeated on many individuals at the most different times, and under the most different circumstances. Frequent repetition of these experiments is the more necessary, the more undecided the expressions of many, even not uninformed persons, happen to be on subjective perceptions. A precise result must therefore throw a more striking light, when, as it is here the case, all patients, without distinction, agree in the most essential points. In order not to confuse by interrogating, I quietly listened to the descriptions of the amputated, rather than examined them; and when they expressed themselves in an indistinct manner, used only to interrupt their story. That, as Fricke remarks, the patients during the operation, on division of the nerves, feel the pain not in the cut part, but in those parts to which the affected nerves pass, is without doubt generally true. This is most distinctly to be observed in amputations of the extremities. Those who, *e. g.*, are operated on in the continuity of the thigh, feel the most violent burning, like that of blazing coals, in the toes and in the ball of the foot, particularly, however, in the calf of the leg. Sometimes they compare this sensation with this, as if the calf of the leg was torn out, as it were, with an hot iron; or, as if the whole surface of the extremity (to be removed) were pierced through with numerous points of small knives. From this sensation appearing in the second stage of the operation, we must, however, distinguish a particular sensation attacking and vehemently shaking the whole body; a mostly burning one, which is wont to accompany the cutting of the skin. That the sensation, as if the lost limb still existed on the body, not only continues for a short time after the operation, but lasts during the whole life, John Müller has particularly dwelt upon with great justice. Macartney has recently asserted, without a shadow of truth, that this delusion occurs only in the moments of pain, for, on the contrary, it does not on any account leave the patient during the whole of his existence for a single moment. If any phenomenon is capable of clearly proving, that certain material sensations, in spite of all the activity of the mind, cannot be removed, that which occupies us at present is one. Although the patients speak of the loss of their limb, observe with their eyes their shortened extremity, or touch it with their fingers, they know without doubt that a part of their body is lost; but in spite of the accuracy of this perception, their general sensation itself, during these moments, is of that kind, as if they were not at all mutilated. Hence the most singular occurrences sometimes takes place; thus nothing is more common than that, *e. g.*, those who have suffered amputation of the thigh, when they go into a cold bed, in winter time, frequently spread out the bed-covering carefully on the side where the amputated extremity

is to lie, as if there was a part of the body still to be preserved from the low temperature. Moreover, when the surface of the stump strikes against a firm substance in its vicinity, *e. g.* the side of a sopha, and thus the feeling of possessing only a stump is brought home to the patient, these unfortunate people are nevertheless unable to divest themselves of the feeling, as if the lost part, which they feel in the liveliest manner, extended itself beyond this resistance, or through it, &c. &c. In this case the conviction obtained by sense and consciousness, along with the feeling communicated by common sensation, are most certainly intimately associated.

It is universally known, that the same delusions prevail also in the dreams of these people; persons who have lost a lower extremity, and make use of a wooden leg or crutches, sometimes dream that they are without, and sometimes with these aids, but never that they are without both extremities, or at least, without being fully conscious that they are able to walk without assistance, like other persons. In such cases the length of time which has elapsed since the operation, and the vivacity and intensity of the ideas in the waking state, appear to have an influence over the general character of their dreams. Thus I have observed, that individuals who have lost a leg or a thigh a long time previously, particularly in the years of their childhood, never dreamed that they were without a crutch or a wooden leg, although it always appeared to them in an equally distinct manner, that they could walk without such superfluous aids. Many persons of this description, even when I had previously directed their attention carefully to the point, denied altogether that they had in their dreams any perception of having lost an extremity, and constantly maintained that on such occasions they had a dim but undoubted consciousness of the existence of the lost limb, and a firm conviction of the possibility of its use. On the other hand, persons who had lost a limb only a few months or years ago, (particularly in the advanced period of life,) dreamed that they possessed both extremities, and were without any aids to walking; nay, they sometimes dreamed that they performed motions which were impossible in the waking state, as dancing, jumping, &c. In the first class of persons, the consciousness of mutilation, implanted at an early period, and fostered for many years, seems to modify in a very remarkable manner the phenomena of their dreams; on the other hand, in the latter, the normal condition which has lasted for a considerable portion of life, and has been only recently altered, appears chiefly to influence the peculiarity of the dreaming perceptions.

That the deceptive feelings of persons who have undergone amputation, are not mere customary sensations associated with certain habitual movements, is particularly shewn by persons who have lost the thigh—the most instructive patients of this description. During the first period after amputation, these persons are accustomed to advance the stump along with the crutch, as if this part had still the power of stepping forwards, and representing one of the supports of the body. It is only those who, in consequence

of their primary disease, have constantly carried the limb in a bent or shortened position, are accustomed to hold the stump quietly in a horizontal or somewhat bent position, so as to enable them to maintain the centre of gravity. But the first mentioned habit is sooner or later lost. The stump hangs perfectly quiet, while the body is swung forwards by the aid of the other extremity, although in all cases the feeling exists in the same way as if the lost limb was still attached to the body. That it is not the limb that remains in perfect integrity which produces the consensual feeling of the integrity of the other, appears from those patients who have lost both legs or all the fingers of both hands. Their sensation repairs the loss as perfectly as it is done by those who have lost only parts of a single extremity.

How little all these deceptive sensations depend on the length of time which has elapsed since the operation, is seen in the cases of those who have spent the greater portion of their lives in this mutilated state. Thus, for example, I have found those sensations as vivid in a person who had suffered amputation twenty-three years previously, as in persons who had been operated on only a few hours. Indeed persons of this description frequently (if not always) have a sense of formication in the lost extremity, which is persistent but slight, and hence not perceived in a sufficiently distinct manner by all. It is only when the mind is engaged in matters of more interest, that this sensation remains for moments or periods without producing distinct consciousness. This phenomenon is observed in the most vivid manner, on the one hand, in persons who possess a finer description of feeling, and particularly in those whose mental culture is of a higher grade: and on the other, in cases where the cicatrized skin lies immediately over the ends of the bone, and consequently prevents the patient from using an artificial limb.

It has been established by numerous experiments, that when pressure is applied to the nerves of the stump, by means of a tourniquet or roller, the sensations of prickling, formication, and sleeping, begin to be felt in the lost extremity. This phenomenon is also observed in the same state of vividness and intensity in persons who have been operated on many years previously, altogether in the same manner as in those whose amputation wounds are not yet cicatrized. The sensation of sleeping is felt first in the foot, then in the leg, and lastly in the thigh, where the patient has lost the lower extremity. If pressure be made on that part of the stump where the sciatic nerve lies, while the remaining surface of the stump continues free from any mechanical pressure whatever, the sense of formication commences first in the great toe; then in the rest of the toes, and the neighbouring portion of the upper surface of the foot; next in the sole of the foot, particularly the inner half; then with peculiar intensity in the heel; then in the calf; next, more gently, in the two inferior thirds of the leg; then in the upper third, close under the knee, rarely in the latter; and finally, in a gentle manner in the defective portion of the thigh; very vividly in

the end of the stump, and the remaining part of the thigh, commencing with the place where the pressure is applied. In general the intensity of the sensation is greatest in the large toe, nearly as strong in the heel, very remarkable also in the calf, and in the remaining portion of the thigh. When the intensity of the sensation is increased by continued pressure, the various degrees of sensation, as given above, are subsequently obliterated entirely by the too great force of the pressure: if merely that portion of the stump where the crural nerve lies, the phenomena in general are more slowly produced, perhaps only with the exception of the sensation in the great toe. First, the sensation is felt in the great toe; then in the rest of the toes, and the neighbouring surface of the dorsum of the foot; then (but very feebly) in the inner part of the sole; on the other hand, at the same time, or shortly afterwards, equally strong in the heel; then moderately in the inner side of the leg; sometime afterwards more intensely in the calf; then in the entire region of the leg; afterwards close under the knee; and finally, after some time, in the thigh and the end of the stump. When pressure is applied to the centre of the outside of the thigh, the sense of formication commences in the great toe; then in the remaining toes; and finally in the heel. On the other hand, nothing more was observed when the pressure was continued for one-half or three-quarters of an hour. Pressure on the inner side of the thigh produces symptoms which resemble more or less those produced by local pressure on the crural nerve, in proportion as the pressure is made on a portion of the thigh situated anteriorly or posteriorly. All this series of phenomena which I have observed, after many experiments, agree in general with those which any person may feel in himself by partial pressure made on an extremity. Probably slight individual differences may occur in such cases, but the progress from the outermost to the innermost ends of the limb, and so on to the end of the stump is universally the case.

What we have seen to occur here in time appears equally to occur in space, or what is rather the case, in point of intensity. Thus several persons who had undergone amputation of the thigh, (and among those the most intelligent and well informed) assured me that they usually thought they felt the lost toes and the balls of the feet occasionally, but less frequently the calves, but never the knee or the lost portion of the thigh. This remarkable occurrence appeared to undergo no change, although the greatest attention was paid to the matter on the part of the patients.

Persons who have laboured under disease of the limb a long time before the operation, particularly those who have laboured under affections of the joints, have subsequently a sensation as if the lost limb was in a more or less bent position. Hence the extremity, which exists only in their sensation, appears to them to be more or less shorter than the sound one.

I have observed this less distinctly, and sometimes not all in two cases, in which, in consequence of sudden mischief, amputa-

tion was performed on the arm and thigh. When the whole body or only the stump changes its position, the lost extremity seems to be moved at the arm or the hip, while the deficient elbow, hand, knee, and foot, appear to remain in a quiet flexed or semiflexed position. This sensation of the position of the limb occurs at the place where it has once existed, nor is ever lost or changed in any point. It even remains undisturbed, in spite of all proof to the contrary furnished by consciousness and sense; and notwithstanding the patient's attention is particularly directed to the matter. Thus a person, aged 20, who in his ninth year has undergone amputation about the middle of the right thigh, for white swelling of the knee, can sleep only on his right side, so that the left lower extremity rests on the stump, in order that the immediate contact by means of the sense of touch, may give accurate information with respect to the limits of the portion which exists, and that which is lost; nevertheless, even under these circumstances, the individual has always the sensation as if the right extremity, which is placed underneath, and seems bent at the knee, was directed outwards under the left.

All these deceptive feelings certainly proceed from the curtailed nervous trunks. Along with the sensations produced by these means, there exist also distinct sensations, as if proceeding in general from the stump, but particularly from the cicatrix. Chemical or mechanical irritation of the latter, provided the ends of the divided nerves are not immediately exposed to it, are felt in the stump and not in the lost portion of the extremity. It is only in cases where the surface is necessarily unequal, in consequence of having only a partial fixed support at the point where the end of the bone lies, that sensations from pressure are readily felt; also at points which are not immediately touched. The difference between both kinds of sensations is seen clearly in those cases in which the ends of the bone are covered only with the skin, owing to the retraction of the muscles.

Thus patients whose stumps are affected with purulent discharges, or contain a portion of loose bone, feel, during change of weather, at the same time, that sensation which has been termed the kalendar, as usual in the lost extremity; and on the other hand, the local pain arising from necrosis, or ulceration, in the cicatrix itself. Sometimes also the sensations produced by change of weather extend from the end of the stump backwards, as far as the point where the hip is connected with the amputated extremity.

Most important Alteration in the Period of Study required to take out Medical Degrees in the University of Dublin.—We are much gratified to learn the advances that *our* University is making in *real* medical reform. It is no longer necessary to spend several years in medical study after graduating in arts, but the degree in medicine may be obtained at the *ensuing* commencements; that is, in *four months and a half* after obtaining the degree of bachelor

in arts, provided the candidate shall have previously pursued his *medical* studies in the accustomed manner, and passed the usual examination. This is unquestionably the most respectable and most useful medical degree in the United Kingdom. The *entire* education and graduation are completed in four years and eight months from entering the University, and the graduate is qualified for any learned profession.

Remarkable Instance of Deformity of the Pelvis.—Professor Naegelé of Heidelberg, has recently presented to Dr. Montgomery of this city, a cast of the pelvis of a woman who was perfectly well formed, and gave birth to five full grown healthy children with the greatest facility. But being unfortunately attacked with malacosteon, and becoming again pregnant, at the close of gestation the pelvis was found so contracted as to leave no possibility of delivery except by the Cæsarean operation, from the effects of which the woman died, and the pelvis was found to present the greatest degree of contraction that has ever been known to require the interference of art.

In the dry preparation the distance between the promontory of the sacrum and the ramus of the pubes, is scarcely a quarter of an inch, so that when covered by the soft parts, these points must have been in contact. The greatest antero-posterior diameter at either side is only an inch and a quarter, and during life could not have exceeded an inch; the sacrum is bent in so as to form a right angle, and the ilia are folded or doubled in upon themselves, like softened pasteboard. Professor N. considers it "the most perfect specimen of contraction of the kind which has ever been made known to the profession."

Cases of Amputation.—In the fifth and seventh numbers of the India Journal, I observe an account of Nipál amputations. Criminals are awarded the same punishment in this part of the world, and the following account of some cases which lately came under my observation may not be unacceptable:

In the month of July last, his highness the Guicwar's minister of justice convicted and awarded punishment to numerous prisoners. Some were condemned to die, others mutilated and set at liberty. On the 21st of July, while several officers were engaged at a pigeon match, the first mutilated object presented himself. He seated himself on the ground, with a clotted stump on each knee, apparently more for the purpose of exciting commiseration than wishing assistance. Both his hands had been chopped off in the morning, and from the town of Baroda he had wandered into camp, a distance of about two miles. I had him conveyed to the hospital, but before he arrived, the distance being considerable, he was for some time exposed to heavy rain. About nine hours had elapsed from the time his hands were cut off to the time he reached the hospital. He looked exhausted and haggard: pulse slow and feeble—no bleeding from the stumps, but occasionally a little se-

rum dropped from the coagulated blood. Nothing had been done to prevent hæmorrhage; nor did he now wish any thing to be done. He was contented to get under a roof where he could escape the rain and die comfortably. I ordered him some brandy and water, and examined the stumps. The left hand was hacked off at the carpus in so bungling a manner, that it had more the appearance of having been torn off than cut. One cut higher than the others penetrated the wrist joint, and injured the ends of the radius and ulna. I had no hesitation in proposing immediate amputation, to which at first he refused to submit, preferring, he said, rather to die, than live a burthen to himself and others. Fortified, however, with some more brandy and water, which he earnestly called for, and which his exsanguined state required, he consented. I removed the arm by the flap at the upper third of the forearm; only the radial and ulnar arteries required to be secured. I afterwards examined the other stump: it was likewise taken off at the carpus, but in a more carpenter-like style: the surface was regular, and probably had been separated at one blow: when washed and wiped with a sponge, scarcely a drop of blood flowed. I proposed to take out the carpal bones at the wrist joint, and leave skin sufficient to cover the ends of the bones. He would not consent to have it done then, and I did not urge him, as he was already much exhausted;—simple dressings were applied;—he had had an anodyne draught and was put to bed. The following day he had much fever, and nothing was done to the right arm.

The fever continued for three days, and was followed by diarrhœa, which nearly proved fatal. Notwithstanding his bad state of health, the amputated arm healed on the first intention, and was quite well in twenty days. The right stump inflamed, and fœtid sanies was discharged from the bones;—granulations sprung up, but so irritable, that the dressings gave him great pain. Finding that the amputated arm healed so quick, and that he could not sleep on account of the pain and irritability of the other, he wished to have it amputated likewise. I took it off by the flap, at the same place as the other, on the 13th of August. A pustular eruption, with inflammation of the integuments of the whole arm, was the only unusual occurrence. He was well and discharged on the 17th of September.

The next case was a man under the care of assistant surgeon Stovell. Two days after the loss of his hand he was taken into hospital. Nothing had been done to stop hæmorrhage, and the only application was a dirty rag to keep away the flies. He never had fainted from loss of blood, nor did he describe the hæmorrhage as much. The executioner had here also mistaken his aim: the hand was taken off just above the wrist-joint. The sore looked healthy, with the splintered ends of the radius and ulna protruding. Amputation was proposed, but he would not consent.

From the 23rd of July to the 16th of August, simple dressings were only applied. Granulations sprung up, but without any appearance of covering the bones, which now protruded, white and

decayed, leaving no doubt but that they must slough before the stump healed. Amputation was again proposed and consented to. The limb was amputated by the flap, in the middle of the forearm, and he was well and discharged on the sixteenth day.

The only other case I have had an opportunity of seeing was a man under the care of assistant surgeon Budds. His hand was taken off at the carpus; the surface was regular, and only two small specks of bone perceptible. He was not received into hospital till some days after he had lost his hand, but the only thing done or applied was, as in the former case, a piece of rag. He would not submit to amputation: nor was it insisted on, as granulations were springing up with every appearance of covering the bones and forming a tolerable stump. Simple dressings were only applied, and three months after, it healed, forming a conical stump covered with tender cuticle.

The above cases, in conjunction with those by Mr. Campbell, in the 7th number of the Journal, prove, evidently, that there is little danger of fatal hæmorrhage from the radial and ulnar arties. In the case under my charge, when both hands were cut off, the bleeding stopped nine hours after the amputation. The blunt hacking instrument employed, by bruising rather than cutting, may have, in some measure, contributed to this. But the chief preventive of fatal hæmorrhage, in my opinion, is the excessive bleeding at first exhausting the strength of the circulation; so that small arteries removed from the heart are not supplied with blood, having sufficient impetus to remove the coagula and prevent contraction of the divided vessels. On this principle do we not bleed in hæmoptysis and other diseases? During the amputation of the first arm, after the patient had taken a considerable quantity of brandy, only two arteries bled, and they were more like the languid flow from a vein than the sharp spurt from an artery. As the arteries of the unamputated stump were not secured, I was apprehensive lest hæmorrhage should take place with the return of a more powerful circulation, and ordered him to be carefully watched. Twelve hours after he had a bounding pulse; but the efforts of nature proved effectual, and there was only a slight serous discharge. The natives of Nipál in their treatment only anticipate nature. They, by tight bandaging, partially obstruct the circulation; so that the blood at the stump is diminished in force and quantity. Dr. Koch's plan, as recommended by Mr. Bramley, is similar; but there is not much of novelty in it,—for, in the beginning of the 17th century, before the use of ligatures were generally known, *Verduin* recommended the flap operation, because the fleshy flap formed an excellent cushion with a bandage to press on the mouths of the bleeding vessels. Among the many late improvements in surgery connected with this subject, *torsion* of arteries, as recommended by M. Amusat, is particularly worthy of the attention of surgeons; and when we know that such arteries as the femoral are effectually secured by simple *torsion*, we need not be surprised at the efforts of nature being sufficient to prevent fatal hæmor-

rhage from such arteries as the radial and ulnar. In the 33rd number of *Johnson's Medical Review* there are several cases of amputation where the arteries were secured by *torsion* with astonishing success. Mutilated arms, on account of the manner in which the Guicwar's executioners perform their duty, must generally require after-amputation, and the above cases show the advantages of it. Amputation on healthy subjects is neither dangerous nor difficult; and the advantages of a good stump, particularly when the patient has been deprived of both hands, are paramount. When left to nature, these cases must require from ten to twelve weeks to heal; and when healed, they are scarcely of any service, the tender covering of cuticle being ruffled by the most trifling injury.

Here, as in Nipál, a common sweeper is the executioner, and both trial and punishment are got over in the most summary manner. The *judge* passes sentence, and the executioner, ready with an adze (*busoola*), blindfolds the culprit, places hand after hand on the block, and at one, two, or three strokes, separates the condemned member. The victim to the offended laws of his country is then released, and at liberty to wander where he listeth, a disgusting monitor to all transgressors. Theft is generally punished with the loss of one hand, but the man whose case is related above, and who lost both his hands, was, I have no doubt, a hardened villain. and, by his own confession, killed a man on account of some ancient feud. These punishments appear barbarous and disgusting in the eyes of Europeans; and the history of Nipál patients, related by your correspondents, shews the futility of them. Nevertheless, if we reflect on the history of our own country, at no very distant period, we may find some apology for the commission of these cruelties by an Asiatic potentate of the present day in *the full exercise* of despotic power.—P. Stuart, *Baroda Camp*, Oct. 9th.—*India Journal of Medical Science.*

On the stimulating Effects of Cold Water.—[The following cases, though partaking not a little of the marvellous, are related as facts by Dr. Davidson of Calcutta, in a late number of the India Medical Journal.]

I have lately read an account of a recovery from the effects of lightning by the application of cold water, in one of the numbers of your Journal, and perhaps you will not object to the publication of the following circumstances, which may be relied on as perfectly free from exaggeration.

I was once called by servants while at Asseergurgh, to kill a large powerful snake that was seen on my farm yard. After a really desperate fight I killed a Dhámun six feet long, and, as I then thought, smashed its brains to atoms with a heavy solid bamboo. I took it up in triumph, and carried it to the front of my bungalow: as I wished to preserve it, and it had become very dirty, I poured a garráh of water over its head, when, to my great surprise, it immediately began running down the hill, and I had to

kill it over again. I believed that the re-animation had been caused by the application of the cold water.

Returning from Bombay in 1823, I passed through the cantonments at Jalná, then occupied by Madras troops. I called one morning on Dr. Alexander, who was attached to the horse artillery, and he told me that he had, in that morning's ride, rode down and killed a sickly wolf with his hunting whip. I wished to see it, and was taken to the compound hedge, where the animal to all appearance lay dead. I said, "I'll make that wolf get up and look you in the face." Alexander laughed, and said that he had smashed its skull with the beavy brass end of his whip, and trailed it lifeless after him for a mile or two. I still persisted that I could re-animate it. At last he said, "Very well, try." Some cold water was procured, and on its being dashed over the skull, the animal got up, rested on its fore legs, and stared at us both with a most horrid glare! If Dr. Alexander be alive, he will probably remember his astonishment on the occasion.

In consequence of these and similar occurrences, I began to place great confidence in the power of cold water applied to the brain; and I will now relate, in my opinion, a very extraordinary circumstance, which shows that it was not without good cause. Soon after my return, one of my chupprassees solicited permission to be absent for a few days, for the purpose of visiting a large melá or fair, held in the middle of a dark and gloomy forest, twenty miles to the east of the fortress. Being fond of seeing sights, I determined to accompany him, and after having despatched a tent, on the second day I rode to a village half way, and slept there during the night; next morning I reached the fair, which was held on an open spot in the centre of the forest. It was a festival in honour of one of those huge striding Hindoo deities, who from the peak of the Hymalaya, placed one foot on the rock in the middle of the fair, and at another step stood on a mountain in Ceylon! There could be no doubt of the fact, as the impression of his foot was still visible on the rock; and it was visited once a year with great reverence by 150,000 innocent "Hindus." Nothing occurred during the day, but in the night I was repeatedly awakened by a simultaneous and universal clapping of hands in honour of the deity before mentioned, which in the stillness of the forest had a very curious and startling effect.

I got up at day break next morning, and mounting my horse, traversed the fair in all directions, at one time mightily tickled by observing faqueers lying naked on the thorns of the bahul (most *carefully crushed,*) and at another admiring the simplicity of the innumerable devotees who were crawling up and down the hill, to visit the sacred spot. On returning home, I perceived at a considerable distance, on the skirts of the fair, two large mobs. I cantered up to the nearest, and instead of seeing some outrageously pious Hindu saint, observed a very stout and handsome young woman of about 20, lying apparently dead on the ground. I quickly dismounted, and found no signs of life, but warmth of body. There was no pulsation at the wrist, temporal artery,

heart or ankle. On inquiry, I ascertained that she had been lying two ghurrees in that state, having fallen down *suddenly* without previous notice or illness. Owing to the mob around her, the heat was perfectly suffocating, and I desired her husband, a grey-headed old man, to clear a large space around her for the sake of air, and I would try to cure her. This he eventually did, with the assistance of other men, but with great reluctance; as he said that she had been so long dead, that he *must take her to be buried.* She was a Mussulman. I proceeded to examine the body more minutely, and all that I could discover was, that the attack was not epileptic, but might be from spasm.

I called out for cold water, but as none was procurable, I despatched my groom, who brought my servants from my tent with a couple of large gharráhs meant for my morning bath, which had been cooling all night. The mob was again cleared, and sitting down, I dashed water on the woman's head two or three times; still there were no symptoms of returning life. I tried again and again, and at last one of the eyelids twinkled. Delighted at this, I cleared the ground again, and uncovering her chest, I poured a whole gharráh over her body.

The effect was magical; she suddenly started up with a scream, wiped the water from her face, seized her dishevelled hair, twisting it behind her head, snatched her clothes, and looked round with a stare of astonishment. A loud shout of surprise rent the air; the miraculous cure of the feringee spread like wild-fire through the fair; accompanied by her sister and husband, I walked to my tent, surrounded by a dense crowd of admirers! After seating the party in my tent, I rode off to the other mob, where lay the body of a boy of ten or twelve years of age, surrounded by howling females. I knelt down, and found his body also warm, but without pulse; I requested that the mob might be cleared, and offered to try my skill on him, and my request was backed by many who accompanied me from the first scene, but all in vain. I was obliged to leave the poor boy to his fate, and was returning to my tent, which I had scarcely reached, when I saw him carried past with his legs dangling in the air, to be, in my belief, buried alive.

I found my patient's pulse very languid, and therefore gave her a pint of old Madeira at a draught, which, "nothing loth," she swallowed, just to give a *fillip* to the circulation! She slept soundly till sunset, when, without once thanking me, she walked out of the tent.

The fair lasted two or three days after this, during which time I never left my tent without being followed by a mob; and I am persuaded that if I had felt inclined, I might have set up for a prophet without further capital, and procured a lakh of chelas in twenty-four hours. But you know that is not my vocation!

A year afterwards, when riding through one of the streets of Booraupoor, I overheard a conversation amongst some Mussulmans, who were disputing, who were the most learned? the

Moosulmans or Feringees? One vehemently backed the Feringees, and swore, "Korán ki Kusum that there were none so clever, for with his own eyes he had seen one of them raise a dead woman to life." "Jooth Bát," said another. I laughed; the Philo-feringee jumped up, and roared out, "Soobhán Ullah, there's the sahib himself, ask him. Krores of people saw him do it."

Since that period I have repeatedly stopped fits of epilepsy in people found lying in the streets in different towns, by the same means.—*Boston Medical and Surgical Journal*, June, 1836.

Cases of Fracture treated by the Bandage, by Professor Dudley. (Reported by James M. Bush, M. D.)—Case I. Judge ——, of Kentucky, was thrown from his horse and sustained the following injury:—each bone of the leg was fractured, some two inches above either malleolus; the tibia lacerating, and protruding a considerable distance through, the soft parts complicated with vertical fractures of the lower fragment. The capsular, lateral, and perpendicular ligaments were ruptured. We have here presented a compound as well as comminuted injury, and, to enhance the difficulty of the case, the patient was a man of the most intemperate habits. Yet amputation was not resorted to. The bones were replaced, the torn ligaments readjusted, and a common roller applied from the toes nearly to the knee. In the injury, the anterior tibial artery received a wound, which, after a few days, gave rise to an aneurism in the part; yet this, together with the fractures, lacerated ligaments, and other soft parts, were all perfectly cured at the end of four weeks.

Case II. A yellow boy, in a state of intoxication, was precipitated from a gig, and received a fracture of the bones of the leg, three inches above the ankle joint, the lower pieces of bone being literally crushed quite into the capsule. He was dressed three times with the bandage, and in three weeks walked upon the leg, as if no injury had befallen him.

Case III. A man was engaged in blowing a well, and after applying fire to his train, being unable to retreat in time, he was overtaken by a large stone which fractured the tibia, and did extensive violence to the soft parts. The roller being applied, a reunion of all the parts was effected in three or four weeks.

Case IV. A negro man endeavouring to mount his horse while his team was in motion, fell, in consequence of being intoxicated, and the wheel of his waggon passed obliquely over his leg. The fibula was broken near its head, and an extensive wound inflicted on the tibialis anticus, the extensor and peroneal muscles. This leg was immediately dressed in the manner already so often described, the edges of the wound being first drawn together by adhesive strips. A cure was effected in a few weeks without the slightest untoward circumstance.

Case V. A man committing depredations upon the watermelon patch of his neighbour, received a rifle bullet through the right carpus, which drove before it the os trapezoides and the body

of the magnum. Some five hours after the accident he was dressed by the bandage alone, one or two additional applications of which were sufficient to render the cure complete.

Case VI. A wood-hauler, in driving his waggon to town, fell from his seat, with his leg across a deep rut in the road. The waggon passed over the limb, and a portion of each bone, precisely the width of the tire, was driven back among the gastrocnemii muscles. The broken fragments were replaced, and the bandage applied. He was then carried ten miles into the country. In ten days, a second dressing was made, which completed a beautiful cure in twenty days.

Case VII. Mrs. —— suffered an injury of the elbow joint, consisting in a separation of the internal from the external condyle of the humerus, the fractured portion being drawn down into the bend of the elbow by the pronating and flexing muscles. Great tumefaction of the parts deceived her surgeon, who mistook the injury for a dislocation, and treated it accordingly. The arm, was cured, the parts remaining in this condition, and stiffness of the joint was, of course, the consequence. The case was presented to Professor Dudley two months after the occurrence of the injury. The internal condyle was found occupying the bend of the elbow, with an ossific deposit within the capsule in magnitude double the size of the fragment. By a violent effort the parts were broken up, the condyle replaced, and the bandage applied. It was afterwards dressed daily, always drawing the roller very tightly over the osseous tumour in the joint; and at the end of fifty days the lady had the satisfaction of seeing the articulation of her joint restored. Now, six months since, the injured limb can scarcely be distinguished. This case exhibits the efficacy of the bandage, as a remedy in fractures, in the most cogent manner. It is an instance of its capacity not only to hold the ends of the bones in apposition, by controlling muscular contraction, but to stimulate the absorbent and nutrient vessels to remove useless parts, and deposit new matter where it is demanded.

Case VIII. Mr. —— was engaged in his hemp factory, when by accident he got the cuff of his coat entangled in the machinery Instantly the hand, wrist, and forearm were all drawn in, and wound around an iron axle. The ring finger was fractured at the first phalanx, the bone being literally crushed; both bones of the fore arm were broken a few inches above the wrist joint, and the soft parts of the finger, together with those of the palm of the hand, extensively lacerated. A suitable roller having been applied to the finger, the hand, wrist, and arm, were put under the influence of a larger one. In this case splints were necessary to keep the radius and ulna separate. At the expiration of thirty days the injured parts were restored.

Case IX. A gentleman received a large ball in the arm, just above the elbow joint, passing directly through the humerus, and separating entirely the external from the internal condyle. Professor Dudley being on the spot, the injured parts were imme-

diately adjusted. The bandage was then applied, and the case treated as an ordinary fracture without regard to the gun-shot wound. The termination was again favourable. No unfavourable symptom presented during the progress of the cure.

Case X. A lady, with her infant child, aged five months, was dashed by accident from her carriage, the latter experiencing a fracture of the thigh bone a little below the trochanter minor. While the little sufferer was yet screaming under the torture of the injury, the limb was gently extended, and a proper roller applied from the toes to the hips, around which it was made fast. Under the soothing influence of the pressure exerted by the bandage, the child at once evinced an alleviation of pain, and soon became quiet. It was nursed upon a pillow, and in twenty days was well.

Case XI. A gentleman suffered a transverse fracture of the patella. It was thus treated:—a bandage was brought with moderate and equable pressure from the toes to near the knee. The upper portion of the bone having been drawn by the action of the extensor muscles nearly half way up the thigh, was drawn down, and held by an assistant in apposition with the inferior fragment. Another bandage being then passed round the hips, was brought down, embracing the thigh and paralyzing the muscles as it descended to the knee, around which it was neatly adjusted so as to fix the bones in their proper situation. The whole limb was kept constantly extended. A cure was accomplished in four weeks. The individual then left town in his gig, and having stopped at a small stream to water his horse, he let fall his glove which was carried away by the current. In his efforts to recover it, his patella, the new attachments of which were still feeble, was again fractured. He was now a considerable distance from his carriage, and in this situation crawled back to it, and returned four miles to town. The same treatment was pursued as in the first instance, and in the same period the fractured bone was a second time united.

The following case illustrates the use of the bandage in a different class of injuries.

Case XII. A man in a personal conflict received a cut on his left shoulder from his adversary's axe, which completely separated the acronium scapulæ from the bone. The arm being elevated at right angles with the body, the parts came into contact. In this position it was maintained by the bandage for twenty days, at which time the parts had united, and the patient was discharged cured.

It were easy to multiply these details, but it is presumed that a sufficient number have been given, to establish the utility of the bandage in such cases. Few days pass in which the remedy is not employed by Dr. Dudley in the treatment of local injuries, and with the most unvarying success. Except in fractures of the forearm, he rarely finds it necessary to use a splint; and in the course of his long and extensive practice, he has amputated but a single leg in consequence of fracture, and that was in the case of a maniac who could not be controlled. In gun-shot wounds, regarded by most authors as *sui generis*, and as requiring, therefore,

a peculiar treatment, he relies with the utmost confidence upon mechanical pressure alone. In amputations, the stump receives from him no dressings except the common roller—no lints, unguents, plasters or tow, interfere with nature in her restorative process; and the consequence is, that no deep-seated abscess, no exfoliations of bone, or tedious stumps, follow his operations.

Such results, it must, nevertheless, be admitted, have not uniformly attended the use of the bandage in the hands of other surgeons. Mortification and loss of the limb, and sometimes of life itself, have resulted from its application. I am apt, however, to believe that this has been in consequence of mismanagement. Those who have attended upon the operations of the gentleman whose cases have just been reported, and have seen how surely by means of it he controls swelling, subdues muscular contraction, promotes absorption, and favours the restoration of injured parts, will not easily be persuaded that mischief can ensue when it is skilfully used.—*Transylvania Journal of Medicine.*

Singular Case of Lead imbedded in the Tibia.—Whilst I was attached to the General Hospital, in Calcutta—says Dr. Atkinson, in the India Journal of Medicine—I had occasion to amputate the leg of an old soldier, in consequence of a gangrenous spreading ulcer just above the ancle. When the operation was finished, we proceeded to dissect the limb in order to examine the internal condition of the diseased parts, and on extending the incision along the tibia, an irregular line of lead was discovered imbedded in the bone, with the external surface smooth. This was several inches above the ulcer, with which it had no connexion whatever, the bone in the intermediate space being quite sound. Upon being questioned on the subject, the man recollected that he had received a wound in action, on the skin, many years before, but had suffered no inconvenience from it, as the slight laceration had speedily healed up, and had left no pain or scar to mark the place, or recall the circumstance to his mind. The quantity of lead appeared to be about as much as might be contained in a musket-ball, scattered and diffused. But how so singular a result could have been produced, it is difficult to imagine. A ball striking the bone would in ordinary cases fracture it; but the resistance in the present instance seems to have been as great as if the lead had struck a dead wall. And it is equally difficult to conjecture how the lead could have acquired its elongated and irregular form. The bony bed in which it lay, was not in any degree exfoliated or diseased; indeed the lead had, as it were, become a part of the man, for the periosteum and skin had closed over it in perfect harmony, leaving no indication of the presence of a foreign body.

Rational View of Small-pox and the Mode of Treatment.—The discovery of vaccination as a preventive of small-pox, towards the close of the last century, afforded a hope that all future speculations upon the nature and treatment of this loathsome disease

would be superfluous. Events, however, have not realized this apparently well-grounded expectation. The small-pox still continues its ravages, and in this vicinity, of late, to an unprecedented extent —more than thirty deaths having occurred in this small city within the last three months.

A total neglect of the preventive, or more often a false security resulting from a spurious or imperfect vaccination, have undoubtedly led to this. The evil exists at any rate; and to the reproach of our art, its average mortality is as great now, perhaps, as it was a hundred years ago.

It is worth inquiry whether some improvement might not be made in the treatment of this formidable disease? If a few observations, which my experience has afforded, will contribute a mite to this object, I shall be gratified by their publication. I have no ambition to write a *treatise* on a subject which has employed so many abler pens. What I have to say will occupy little time or space.

The small-pox I hold to be essentially *an inflammatory* disease; whilst the prevailing opinion with physicians seems to be, that all bad cases of it are to be considered as typhoid or asthenic. The influence of such an opinion on the practice is obvious. It was the doctrine taught by Cullen, and that which has continued to govern the practice more or less, to the present day, that whilst the fever attending the distinct small-pox was admitted to be inflammatory, that which attends the confluent was to be considered typhous, malignant, or putrid, and stimulants and tonics freely employed in the treatment. More than forty years ago—*horresco referens*—I was convinced, by abundant observation and experience, of the fallacy of this doctrine, and that many lives were sacrificed to the practice which it indicated. External appearances, it is true, the sight and the smell of a patient labouring under confluent small-pox, from the ninth to the thirteenth day, for instance, suggest the idea of a highly putrid disease; whilst blood drawn, (as it often should be,) to relieve some urgent symptom, shall exhibit the inflammatory buff, and by this, and the relief it affords, prove how far we have been deceived by appearances.

I am aware that the free admission of cool air, the use of cold drinks, and generally an antiphlogistic treatment, have been recommended and employed by most physicians, to a certain extent, from the time of Sydenham, and in all ordinary cases of distinct small-pox, with a safe result; indeed, these cases are attended with comparatively little danger, and require little medication. It is in the worst forms of the disease, only, and in those cases alone which threaten the life of the patient, that the skill and active interference of the physician are demanded. In the treatment of such cases, attention, as I conceive, has been too exclusively bestowed upon the state of the pustules, and their progress towards maturation, the swelling of the face, and successively of the hands and feet, the ptyalism, &c. whilst the degree of fever, the condition of the head, throat, and lungs should be more especially regarded.

The small-pox, I repeat, is emphatically, and in this climate

especially, an *inflammatory disease;* and when it proves fatal, it is by excessive irritation, or the occurrence of some local inflammation of the brain, throat, and lungs. To obviate or allay these, is the chief concern, and generally speaking, with the means employed to effect this, the skin will take care of itself.

The terms *distinct* and *confluent* are no farther useful than as they mark the two extremes of a disease, which occurs in every intermediate degree, but which is in itself one and the same. Indeed, the primary, or, as it is called, the eruptive fever, is sufficient, in three or four days, to work that mysterious change in the constitution which, without a single pustule, shall protect it against any future attack of the disease; in short, the patient shall have passed the small-pox. Of the truth of this, we have abundant evidence in the practice of inoculation, and it is highly probable that some have experienced this process in the natural way, without suspecting the character of their short-lived indisposition. But an eruption of pustules so generally follows this temporary fever, as to be considered essential to give "a local habitation and a name" to the disease. And as we know the danger of it to be in a great measure proportionate to the number of these pustules, the irritation they cause in their progress, and the sympathetic affections they are likely to induce; when we have a well-grounded suspicion of the nature of the patient's illness at an early period, much may be done to lessen the number and size of these pustules, the quantity and quality of which, it is believed, depend very much upon the state of the skin at the time. The state most favourable to a kindly eruption consists, probably, in a just medium between the extremes of torpor and irritability of the capillary vessels, and an excess of the latter is the most frequent cause of an aggravated disease. Hence the advantages of cool air, cold drinks, cold effusions and gargles, keeping out of bed, a spare diet, and depletion by blood-letting or cathartics. The latter, although highly recommended by Friend, during what has been called the secondary fever, and by Tissot at a somewhat earlier stage of the disease, has not, as far as my observation goes, been duly appreciated during and immediately after the eruptive fever.

This is the first improvement I would suggest in the practice—having so often witnesed the efficacy of cathartics in lessening the number, size, and soreness of the pustules, and converting, what by an opposite course would prove a confluent, into a distinct and manageable small-pox.

The fear of hindering the eruption, of causing a retrocession of the pustules, or of weakening the patient too much for *filling out the pock*, has, I think, rendered practitioners over cautious in the use of this class of remedies at an early period of the disease. The pustules, in all ordinary cases, will be found sufficiently prompt and persevering; and it should be remembered that there may be too much, as well as too little inflmmation for a kindly suppuration of phlegmonous tumours.

When, in the progress of the disease, delirium, inflammation of

the throat or lungs, with pain and difficulty of deglutition or breathing, supervene, bleeding, either general or local, should be resorted to with freedom and without delay. Leeches applied to the throat will be found eminently useful—and here, again, cathartics should be resorted to freely. Calomel, with its combinations, should be preferred, and the same in small doses as an alterative, through the whole course of the disease, will in most cases be advantageous, whether as an antidote to the virus, as some have supposed, or by its action on the fauces, and peculiar influence on the suppurative process.

Cathartics will be found the most powerful means of relieving the distressing fulness of the throat, and, at the same time, like the diarrhœa in children, prove in some measure a substitute for the ptyalism on which so much stress is laid. When, from hypercatharsis, or any other debilitating cause, a paleness of the surface with faintness should occur, a small dose of laudanum with lavender may be given ; but, generally speaking, stimulants or tonics should be administered with great caution. Wine or bark is seldom required in small-pox. There may be moments of temporary depression and *sinking of the pustules,* where a little wine may be admissible ; but, even here, small doses of the sulphuret of antimony, or the common flor. sulphur, will be found preferable. They act on the capillary vessels of the skin without sensibly affecting the general circulation.

When that exacerbation of fever, called the secondary fever, takes place; whether from absorption of the matter of the pustules, protracted irritation, or local inflammation. or some unpublished law of the disease, I shall not theorize, it is sufficient to say that we still have to deal with an *inflammatory* fever. Blood-letting will often be necessary, and cathartics always. The latter, I suppose, have been very generally employed by practitioners in this stage of the complaint, but seldom, I suspect, with the freedom they ought to be, or as practised by Friend, who first recommended their use, a hundred years ago. The misfortune has been, however, that the good effects of this remedy, when prescribed, have been counteracted by wine, bark, and opium. The two former add fuel to the fire, and the latter, when given indiscriminately, as advised by some eminent authors, is liable to do harm by promoting congestions in the brain or lungs. There will, however, be cases of peculiar irritation and restlessness, where, after the bowels have been duly evacuated, opiates may be given to advantage, but always with due regard to the state of the system. It is better, as Tissot implies, that the patient remain wakeful a few nights, than dry the throat, exasperate delirium, or induce coma, by a too liberal use of opium.

The mineral acids, preferring the sulphuric, will be found more applicable, and should be freely given, properly diluted, in this and every stage of the disease.

But, as I premised, it was not my intention to write a *treatise* on the small-pox ; my chief object was to correct what I believed to be an erroneous opinion, too generally entertained, of the nature of the

disease—to recommend a bolder use of cathartic medicines, and to discourage that of tonics and stimulants. As to details—such as the opening of the pustules, to lessen, as far as possible, the quantity of matter to be absorbed—the great advantages of cleanliness, a hard bed, a well-ventilated room, and the attentions due to many particular symptoms, are they not already written in the books of the chronicles of this disease? I will only add that the small-pox, with its *mimosis* or fellow-traveller, the varioloid, still continues to prevail in this place.—L. W.—*Boston Medical and Surgical Journal*, June, 1836.

On the Physiology of Hearing, by Dr. Steinheim.—Seeing and hearing singly, with double organs of sense, belong undoubtedly to the more difficult points connected with the functions of the senses. It is well known that the single sense of touch is lost by crossing the fingers; for a little ball, as for instance a pea, communicates the sensation of two, when it is rolled backwards and forwards between the two first fingers crossed.

Something quite analogous to this takes place with respect to the sense of sight, in cases of absence of mind.

The object of sight separates gradually into two distinct images, an affection which is not unfrequently observed in cases of abdominal derangement. As when the fingers are crossed, the usual sentient association is interrupted by changing the position of the nerves employed, and in such circumstances each sentient nerve conveys its individual impression to the mind; so when the direction of both eyes to a single object is interrupted, the ordinary position of the optic nerves is changed, and each becomes sensible to the individual impression. If we now reflect, that in both the cases alluded to, it is the business of consciousness to receive the double impression as a single one, (in consequence of which the error produced by the unusual position of the nerves of touch, is, after a few experiments repeated in quick succession, corrected,) the impression made on the senses is, without doubt, in every instance double with respect to the three objective senses—touch, sight, and hearing.

This phenomenon is universally known with regard to the two first senses; the same thing remains still to be proved with respect to the sense of hearing; namely, "THAT EVERY SOUND IS HEARD BY EACH EAR SINGLY, AND MAKES TWO IMPRESSIONS WHICH BECOME BLENDED INTO ONE.

In order to perceive this phenomenon, the following procedure should he adopted. The experimenter strikes two quarter notes with two fingers of the left hand, and at the same time a half note with one finger of the right hand; if he now listens attentively to the sounds produced by the right and left hand, he will hear four instead of three sounds; provided the notes are struck in such a manner that the stroke of the right hand finger falls between the two strokes of the left, which are only half as long as that of the right; that is to say, on the left a triole (000) and on the right a

single stroke, and the latter will be thus doubly heard, between the two quarter tones, and at the same time individually as a half note.

Now, we cannot well imagine that the note produced by the right finger is heard twice by one ear; it remains then only to assume, that it is perceived by the right and left ear separately.

It may not be here out of place, to communicate some observations on seeing, which belong to this point.

Any person whose eyes have different focal distances, may easily repeat them, as I have frequently done. Persons are seldom aware of the unequal power of the double organs of sense, unless it be very remarkable, or happen to be discovered by accident. I know a man, who became for the first time aware of an amaurosis of the right eye, on looking one day through a telescope, and finding that he saw nothing. He cleaned the glass, but in vain, and then for the first time became conscious that he had lost the sight of his right eye. With regard to myself, I was informed of the twofold difference between my right and left eye, in the following manner. I happened to be in a rather dark room, opposite a door, at the moment when the opposite illuminated wall was seen distinctly through the keyhole. The bright image of the keyhole separated into two, and these two very dissimilar images. The image of a large bluish keyhole presented itself opposite to my left eye, that of a small yellowish white one opposite to my right eye; in the larger one the lines of the opposite bright wall, which shone through, were confused together; in the smaller the same white lines were sharply defined. Further trials of my sight, by looking at a white surface with each eye alternately, convinced me that my right eye was *myopic*, (short-sighted,) and saw objects large, and of a blue colour; and that my left eye was *presbyopic*, (long-sighted,) and saw objects small, and of a yellowish colour.

But how then did it occur that the image of the keyhole opposite my left eye appeared larger, more indistinct, and bluish; while that opposite my right eye seemed smaller, more sharply defined, and yellowish. It was produced by the crossing of the image, for in this way the image seen by the right eye was to the left of that seen by the left eye, and *vice versa;* and hence it happened that it must have appeared to me as if both eyes had exchanged their mode of seeing.

In this way I explained to myself this singular optic phenomenon. There is another, which up to the present day is still to me a riddle. It is well known that we can assist a short-sighted eye by diminishing its field of vision. Short-sighted persons instinctively draw their eyelids together, or look through a small opening between the index and middle finger, formed by laying their tips perpendicularly on the thumb nail. This artifice is of great help to my right eye, but it also assists my left eye, which is long-sighted, and that is to me a problem, the explanation of which I cannot even conjecture. As distant objects appear clearer and more distinct to my right eye, when I look at them through

the small triangular opening formed by the three first fingers; in the same way near objects become distinct to my long-sighted eye by the same mode of proceeding.

Another phenomenon, which seems to me worthy of attention is, that from two different images seen by each eye separately, a single homogeneous one is produced, in such a manner that with respect to distant objects the colouring of the far-sighted; and with respect to nearer objects, that of the near-sighted, preponderates. Again, the power of vision in both cases is diminished, when one or other of the eyes is closed. Near objects, which I can no longer discern with the left (far-sighted) eye, and consequently must be seen with the right, become still more indistinct when I close the left; and I also see distant objects more clearly with both eyes than with the left alone, with which alone, however, I am able to discern distant objects.—*Translated from Hecker's Annalen*, 1836.

Effusion of Blood into Chambers of the Eye from Injury, by Edward J. Davenport, M. D.—The following case is offered chiefly to exemplify the extraordinary power which the aqueous humour possesses in dissolving and promoting the absorption of foreign substances in the anterior chamber of the eye.

H. Merrifield, aged 22, stone-cutter, received a violent blow upon the corner of the right eye, on Wednesday noon, followed by immediate loss of vision, with pain of the eye and forehead. He was seen two hours after the accident, at which time the tunica conjunctiva had become injected, and the eye was tearful; the two lower thirds of the anterior chamber were filled with blood, of which the lower third appeared to be coagulated, while the upper was partially diffused or mixed with the aqueous humour; the pupil was widely dilated, presenting a clear and transparent space above the volume of blood; the cornea was opaque at one small spot only, and at this point the surface was broken or abraded; vision was nearly or quite extinct, the patient retaining, with this eye, merely the perception of light. This diminution of vision was not owing entirely to the presence of blood in the anterior chamber, but rather to some deranged condition of the retina, simultaneous in its occurrence with the sanguineous effusion, and referrible to the same cause, viz. concussion of the eyeball. This may be inferred from the fact, that there existed at the upper part of the dilated pupil, above the effusion, a sufficient space to admit the rays of light. The pulse was not affected, and the patient absolutely declined having any blood taken from the arm. He was therefore directed to have leeches applied freely upon the right temple; to take one of the following powders every third hour, until active catharsis should be induced; to keep the eye covered with cloths constantly wet with cold water; and to adopt a strict antiphlogistic diet and regimen, with entire rest for the eye.

R. Magnesiæ sulphatis ℥ss. Jalapæ pulveris ℈i. Misce.

Thursday, 2nd day.—The leeches had bled freely, and the patient had been actively purged by the cathartic medicine, by which means the pain of the forehead and eye, previously very severe, had been removed. One-half of the effusion of blood had been absorbed (in about eighteen hours from the time of the accident,) and vision had become somewhat more distinct. The pupil, however, remained in the same state as on the day previous, and the conjunctiva continued to be vascular. Leeches and cathartics were directed to be repeated.

Sunday, 5th day.—The effusion of blood had nearly disappeared from the anterior chamber, leaving merely a coagulum at the lower part, similar in form to the collection of pus seen in the early stage of hypopium. Power of vision had now so far become improved, that the patient was able, with the injured eye, to distinguish the hour by a watch. The pupil still remained dilated and insensible to the stimulus of light, being in a state of paralysis from the pressure of blood upon its fibres. Some degree of haziness or dimness of the pupil was perceptible, but the pain, and likewise the vascularity of the eye, had abated. The patient now preferred to have warm topical applications to the eye, instead of the cold lotions which had previously been more agreeable to his feelings. To promote, by its sorbefacient powers, the portion of blood remaining in the anterior chamber, and also to restore vision perfectly, the patient was advised to take a small quantity of calomel and opium night and morning.

Sunday, 12th day.—The patient had taken the pills three or four times only, and had returned to his work some days since, considering the eye as being entirely well. His mouth had not been at all affected by the calomel. No trace of blood was now visible in the anterior chamber, and the conjunctiva was not injected. The pupil continued in a state of dilatation, and the iris was very sluggish in its *lower* half, where that membrane had received the weight and pressure of the blood, while the upper section had recovered motion and activity. Vision was perfectly restored. The iris very gradually recovered its tone and sensibility to light, and no further inconvenience was experienced.

The power of dissolving extraneous matter,* such as lymph, pus, blood, &c. with which the aqueous humour is wisely endowed by nature, has been in some rare instances sufficient to remove the points of steel instruments accidently broken off and left in the anterior chamber, in operations upon the eye. The solution and consequent absorption of portions of the opaque lens is familiar to every one, and beautifully illustrates the application of the conservative vital power, in removing any impediment to the perfect exercise of functions so essential to our comfort and even existence.—*Ibid.*

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* With regard to this power of solution and absorption acting upon effused lymph, some observations, worthy of attention, may be found in a Treatise on Diseases of the Eye, by W. Lawrence, Surgeon to the London Ophthalmic Hospital, p. 248.

Case of Introsusception, in which an Operation was successfully resorted to, by John R. Wilson, M. D.—The subject of the operation was a Negro man, aged about twenty years, the property of Mr. Charles Dement. The patient had laboured for seventeen days under bilious colic, and stercoraceous vomiting, and the other more alarming symptoms of this disease, had appeared. All the active purgatives were administered in vain, and on the evening before the operation was resolved upon, as a *dernier resort*, some ounces of crude mercury were given. The constipation remaining, with the other formidable appearances, it was plain that nothing but the knife could save the patient.

The operation was performed in the following manner: an incision was made along the linea alba, commencing above the umbilicus, and extending two or three inches below it, being in all about five inches in extent. The bowels being protruded through the wound, that portion involved in the stricture came into view. It was found to be in the *ileum*. The bowel was grasped above and below the point of obstruction, and after several efforts of considerable force, the adhesion gave way. The exertion necessary to break up the attachments, it was feared, might lacerate the intestine; but no such accident followed. The bowel strangulated was of a dark, livid appearaece, evidently approaching to gangrene, and of double its ordinary size. The vessels of the omentum were also deeply engorged with black blood, apparently stagnant. The parts seemed to be on the verge of mortification. After returning the intestines into the abdomen, having carefully excluded the atmosphere during the operation by a warm, moist cloth spread over the viscera, the wound was made secure by a few stiches with the needle, and adhesive strips. The patient was put to bed, and in a very short time voided the mercury which he took the evening before. His recovery was rapid and entire.

The success of this case, in which the operation was so long deferred, and at last performed under such unfavourable circumstances, warrants the propriety of resorting to it in the disease, and proves that relief may occasionally be afforded by this means, when all others have failed.—*Transylvania Journal of Medicine.*

Experiments in Animal Magnetism, by Benjamin H. West.—Knowing that your columns have been open to remarks on Animal Magnetism, I beg you to have the goodness to publish the following article, both for the sake of science and the cause of philanthropy.

The experiments and operations herein mentioned, were performed on a girl of the age of twelve years and a half, and who has been affected with epilepsy ever since her sixth year. She is under the care of M. Bugard, a French teacher in this city, and *not* a professor of magnetism, who disinterestedly uses his magnetic powers merely for the benefit of his fellow-creatures, and in philosophic investigation. The patient had been magnetized thirteen times, often in presence of Drs. Ware, Lewis, and Glover, of Boston. She certainly has been affected by the operations, and apparently favourably.

That she is under the influence of magnetism will be evident from the following statement.

On the 27th inst. Dr. Ware meeting Mons. Bougard, inquired of him concerning the health of his patient. Mons. B. replied that he thought her better; and from what I can see, he continued, I think that magnetism has great power over her, inasmuch as she seems to have no sense of feeling during the operation; to-morrow we intend to extract a decayed molar tooth.—Do you not think that she will awake? asked the doctor.—I do not know; but since, in France, much more severe surgical operations have been performed on persons under its influence, *without producing pain*, I think we may succeed in this case.—I assure you she will awake, said Dr. W.—I do not know, replied Mons. B.—I have no doubt of it; however, I should like to see it.—I should be happy to have your attendance.

On the next day the contemplated operation was performed by the writer, in presence of Drs. Ware and Glover; not only without awakening her, but without even the slighest indication of feeling on her part. After she awoke, ten or fifteen minutes having elapsed, she conversed with us for several minutes without any reference to what had taken place. These circumstances, so extraordinary, both from the age and sex of the patient and the nature of the operation, led to the invitation of several scientific gentlemen to witness the operation of this day. We subjoin the history of the seance of the 30th of June, at which Professor Treadwell, of Harvard University, Drs. Ware, Lewis, Harwood, and Lodge, A. D. Parker, esq. and Messrs. Ware and West, medical students, were present.

Pulse, before magnetizing, ranged from 84 to 90. Mons. B. began at nineteen minutes before ten. The patient immediately complained of feeling sleepy. At the end of three minutes, Dr. Harwood and Mr. Barker entered the room; at four minutes, the tickling of the lips, nostrils, and ears of the patient produced no motion; the right eyelid was raised, the lower lip drawn down, without her notice. Five minutes having elapsed, Mons. B. addressed her. Louisa?—Sir.—How do you feel?—Sleepy.—Do you want very much to sleep?—Yes.—How long have you slept?—I don't know.—Holding his hands before her face, Do you feel my hands?—No.—Louisa, let me see your tongue; patient complied with the request, and put out her tongue. Seven minutes: was requested by Mons. B. to open her mouth, which she did, and tooth was examined. Dr. Ware: Louisa?—Sir.—How do you do?—Very well.—Do you hear anything?—No.—No noise in the street?—No.—(A carriage was then passing.) Prof. Treadwell: Louisa, where are you?—In this room.—What room—Mons. Bugard's. Nine minutes. At this time the pulse were 100 per minute. At ten minutes, Mons. B. asked how she felt, to which she replied that she was *sleepy;* she complied with his request that she should open her mouth. Dr. Harwood then extracted one of the molar teeth (it being deciduous and decayed). Dr. Ware perceived no change of pulse during the operation, although there was a flush over the whole face, and a slight quivering of the lip, with a counte-

nance indicative of considerable pain. Mons. B.: Louisa, how do you feel?—I feel the tooth.—Have they pulled a tooth for you?—Yes.—Who did it for you? Dr. Ware.*—Why do you mention Dr. Ware?—I don't know. Fifteen minutes: pulse 96. How happened it that you did not cry, asked Mons. B., when a back tooth was removed?—I never cry on such occasions. Spits out the blood at the request of Mons. B. During all this time the patient remained apparently sound asleep. Seventeen minutes: How do you feel?—Sleepy. Nineteen minutes: Mons. B. Having magnetized some water, gave it to the patient, who drank it. How do you like it?—Very much.—Very much?—Yes.—How does it taste?—Very good.—Better than common water?—Yes.—Do you find any particular taste in it?—No, I find it very good. Says she does not remember having drunk any water during yesterday's seance, it having then been given to her. Twenty-two minutes: motion of lips on irritation with a feather. Twenty-five minutes: the magnetizing of the flexors of the right arm, produced a slight flexion of the thumb and fingers. Mr. Parker clapped his hands very near her ear, when a slight action of the eyebrows and a flush of the cheeks ensued. Mons. B. played on the flageolet; Dr. Ware asked her if she heard anything?—Yes.—What?—Music. Mons. B.: Do you like it?—Yes. Dr. Ware played. Mons. B.: Do you hear anything?—Yes.—What?—Music. Says she likes it, but that it will wake her. Mons. B.: Are you weaker?—No, I think I am getting well: I think magnetism will cure me. Thirty-three minutes: lips and nose tickled without motion. Dr. Ware: Do you feel anything?—No.—Nothing?—No. Thirty-five minutes: Mons. B.: Louisa?—Sir.—Awake, I wish you to rise. She did so immediately. Did you dance?—No.—Did you hear music?—Yes. Dr. Harwood: Did you feel anything about your mouth?—Yes.—What?—Tooth out; I know my tooth is gone by my tongue and the blood. Dr. Lewis: Who extracted it?—Don't know; one of the doctors.—You are positive you have been asleep? Yes.—How long do you suppose you have you slept?—Don't know. Dr. Harwood: Did it hurt you a little?—Not at all. Dr. Lewis: Then you did not know anything about it?—No. Prof. Treadwell: Did it hurt you?—No.—Did you not know when he pulled it?—No.—How do you know it is gone;—Because my mouth is bloody only when my teeth are pulled.—How do you know there is any blood?—I perceive it in my mouth.

If it be indeed true that animal magnetism has this powerful effect, and that it enables the patient to undergo surgical operations with so much ease (in support of which hypothesis, vide Cloquet's operation for cancer in the breast), the subject must be worthy the close attention of every physician, especially of every kind-hearted surgeon; and particularly since the direct exertions of the Faculty will be almost the only mode of preserving us from the invasion of myriads of

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* Dr. Harwood entered the room after the patient was asleep; and had never been seen by or named to her.

ignorant knaves and impudent quacks.—*Boston Medical and Surgical Journal*, June, 1836.

We have given the above for the satisfaction of those who are inclined to believe in animal magnetism. It is quite consoling to find incredible facts solemnly attested by competent witnesses!—EDITORS OF THE DUBLIN JOURNAL.

Flagellation in Cases of Poisoning.—[A correspondent, a professor in one of our medical schools, has directed our attention to the following case in the third volume of the Medical Repository, published in New York in 1800, which was communicated by Dr. Valentine Seaman, of that city. It will be perceived that the treatment was similar to that used in Dr. Barratt's case, published in No. 13 of the Medical Journal, although we have no doubt that the idea was original with Dr. B.

The following is the substance of the case referred to, which is published merely as an additional proof of the value of the remedy so successfully used by Dr. Barratt.]

"The wife of ——— Head, in Water-street, had about two hours before swallowed ʒj laudanum, and then lay in a deadly stupor, from which all the efforts of her friends could not awaken her. Attempts had been made to get some vinegar into her stomach, with little effect; nor did I (says Dr. S.) succeed much better in endeavouring to give a dose of white vitriol. I then procured a small switch, and applied it pretty freely to her arms and shoulders, which were defended only by a thin covering. I also applied some strokes to her legs. In the course of a short time, indeed almost immediately upon the application of the remedy, she roused up, and begged me to desist. She continued for a time much confused, with involuntary turns of laughter. Two scruples of white vitriol were then administered, followed in about fifteen minutes by ʒss. ipecacuanha; notwithstanding which, and having tickled her throat repeatedly with an oiled feather, it was nearly an hour before she could be made to puke; however, finally she did puke, and by the assistance of frequent draughts of warm water, her stomach was pretty thoroughly evacuated. By the assistance of her friends she was kept awake; or, at least, slept but little at a time, during the night, and this [next] morning appears entirely recovered."—*Ibid.*

Account of a Remarkable Misplacement of the Stomach, by Dr. Bright.—*Examination after Death.*—The body was considerably emaciated, and the left side of the thorax was decidedly fallen in: the form of the chest was likewise remarkable, from its great length, the ribs reaching within a short distance of the crest of the ilium. On removing the sternum, the heart was seen lying in its pericardium almost on the right side of the chest, being considerably beyond the middle line; and on its left lay a tumour of a red or fleshy colour, somewhat vascular, slightly lobulated, and elastic to the feel. This tumour rose to the fourth rib; and a small portion of the lung

appeared overlapping it from above. On the right side, a thin flap of the lung descended to the seventh rib, lying upon another tumour nor unlike that on the left cavity of the chest. In the abdomen, the liver occupied a considerable space, extending a little more to the left side; the stomach was no where to be found; while the arch of the colon was seen passing across the abdomen, just below the liver; and an elastic tumour arose from under the acute edge of its right lobe.

On further examination it appeared that the two bodies which encroached on the right and left cavities of the thorax were formed by the stomach contained in a membranous bag, over parts of which a scanty distribution of muscular fibres could be traced: when this bag was cut open, the stomach, covered with the omentum containing the usual deposit of fatty matter in its meshes, came into view. The œsophagus was now traced; which commencing in its natural way, terminated in the cardiac portion of the stomach, on a level with the body of the fourth dorsal vertebra.

The stomach, after filling all the lower part of the left cavity of the chest, passed behind the apex of the heart, where it was somewhat contracted; and again expanding, filled in the same way the lower portion of the right cavity, but to a less extent: the canal then passed through the diaphragm, in a situation nearly corresponding with the passage of the cava; and the duodenum running in a straight line downwards, behind the liver, was involved in a fold of peritoneum, where it was connected with the head of the pancreas, and accompanied by the cystic, hepatic, and common bile-ducts: the intestine then dipped behind the mesentery and meso-colon, and pursued very nearly their natural course.

The cyst which enveloped the stomach was a membranous muscular covering, not in any part adherent to the stomach; and appeared to be formed by a congenital splitting of the diaphragm; for on both sides we could distinctly trace bundles of muscular fibres coming off from the part where the diaphragm arises, and passing above the stomach towards the mediastinum; where they became so attenuated, and the cyst so membranous and mingled with cellular membrane, that all traces of muscular structure were lost. On the left side, there seemed to be even a third division of the diaphragm —in which we observed no muscular structure—going to envelope the spleen, and surrounding it by a membranous bag, out of which it could be turned, like the heart from the pericardium. The spleen was small, hard, and fleshy; but we unfortunately neglected to trace how it was connected with the stomach, or what course any of the splenic vessels took. The pancreas appeared in its usual position, relative to the spleen.

The stomach was very large; and the fluid it contained nearly filled a wash-hand basin. Its internal surface was throughout of a dusky red, and its texture generally thickened; but in the part corresponding to the small curvature were found two or three deep chronic ulcers, with raised edges of almost cartilaginous hardness, one of which would have contained a small walnut in its cavity. The pylorus was very large and open; and was, I believe, situated just

above the diaphragm. The duodenum was greatly dilated at its origin, and partook of the appearance of the stomach.

The elastic tumour which we observed, below the margin of the liver on the right side, proved to be the pelvis of the kidney, distended, to the size of a tea-cup, with clear urine; and the kidney itself was much diminished by absorption. The left kidney was perfectly healthy.

For the facts contained in the following history of the case I am indebted to Mr. Joseph Ridge; who having watched it during life, and assisted in the examination, collected the history, with much care, from the relatives.

Sarah Charington, aged 19, was very delicate from her childhood. At three months of age she had a severe attack, which was attended with sudden shortness of breathing, and was considered an inflammation of the chest. When nine months old she had vomiting to an alarming extent; and when four years of age, suffered a most severe attack of the same kind, accompanied by purging; and the medical man who then saw her pointed out the peculiar conformation of the chest, and the little chance that the child would reach to years of maturity. Before the age of five she had passed through the measles and the hooping-cough pretty favourably, and at eight years had a severe attack of fever. During her childhood she was always unable to play with other children, or bear any rough treatment; and, on several occasions, was expected to die from sudden dyspnœa. Her stomach was always liable to be deranged by the slightest cause; and she never had a figured motion till fourteen years of age. About that time, however, her bowels became costive; and in that state they ever after continued. She grew fatter and stronger, and had some colour in her cheeks: nevertheless, she enjoyed no good health. Her appetite was capricious; she could scarcely walk upright; and if she lay on her left side, it produced great uneasiness, and frequent vomiting: she was most easy when lying on her back, with her shoulders raised and her knees bent towards her body, or sitting doubled on a chair: she had frequent palpitation, which she used to compare to the fluttering of a bird in her chest. The catamenia appeared once, at the age of seventeen; and only twice afterwards, at very long intervals: there was frequently a slight leucorrhœal discharge.

Her mother states, very decidedly, that when suffering from want of food, or from repletion, and previously to sickness, she referred the sensations of nausea, sinking, pain, or tension to the left side of the chest and the sternum; and that there was a frequent rattling or gurgling noise in the side of her chest, which she has often observed during her sleep.

At the age of fifteen, owing to her inability to perform laborious work, she was taught a sedentary trade; but three years ago she came to town, and engaged as a servant: after, however, trying three or four places, she found herself quite unable to continue in them: the kneeling position of scouring the floor was always very easy and agreeable to her, but any other exertion brought on dyspnœa

and sudden attacks of sickness, which often seized her in the street, or on the stairs, when sent on any message; and though at that time rather cheerful in disposition, she found it impossible to persevere. She often complained of pain between the scapulæ, and a stabbing sensation from the lower part of the sternum to the spine; and of a fullness and tightness in her left side. It was with difficulty she raised her arms so as to place her hand behind her head. Her symptoms were all subject to great changes and varieties; and she had what she called her good and her bad days; but the periods of comparative comfort were but short.

Towards the end of April 1835, she was seized in the street with violent sickness, vomiting, a large quantity of brown matter; and after that time she was more subject to vomiting than before. She lost flesh, and became obviously more out of health. She was sent down to Ipswich in July, to be put under the care of Mr. Sampson, from whose advice she had formerly derived much advantage. She returned to town decidedly improved; but soon becoming worse, was admitted into Guy's Hospital, under my care, October 14th, 1835. At that time she appeared of delicate frame; and was evidently emaciated, complaining of constantly vomiting her food, and feeling sick several times in the day. She was unable to lie on her left side, on account of the vomiting it produced. She complained of pain at the pit of the stomach, increasing much at times. Bowels much confined: leucorrhœa: amenorrhœa: no cough: pulse 100.

Bismuthi Subnitrat. gr. v. Extract. Gentian. comp. Extract. Papav. alb. āā gr. ij. ter die.

Oct. 15th. She had vomited three or four times since her admission: in other respects, was much the same: but on examining the chest, I found that the pulsation of the heart was quite distinct upon the right side, while it was scarcely to be perceived upon the left; and though the resonance of the chest was clear, there was little or no respiratory murmur in the lower part. The bowels not having been opened, a mixture, with magnesia and the sulphate of magnesia, was ordered to be repeated till the full effect was produced.

16th. Vomiting diminished, and she felt better:—and now, for the first time, she mentioned that many other medical men had observed that her heart beat upon the wrong side, and that such, she believed, had always been the case. I found the sounds of the heart to be those of a natural, somewhat irritable organ. The urine was loaded with the lithates and was not coagulable by heat.

Inject. enema catharticum, statim.

19th. Vomiting has nearly ceased: bowels pretty freely open: urine loaded with the lithates.

Rep. Pilulæ et Misturæ.

23rd. After exerting herself a good deal yesterday, the sickness returned slightly.

Sumat Aquæ Calcis ℥vi. ex Lacte quotidie.

Nov. 3rd. A slight return of sickness the last two days.

Cap. Acidi Hydrocianici m ij. ex Misturâ Mucilaginosâ, ter quotidie.

7th. The sickness having now subsided, she was able to walk about gently; and being apparently in her usual health, she was permitted to return to her mother.

Three days after leaving the hospital, she imprudently undertook a walk to Hackney. This brought on the sickness again in a most violent degree, so that she very generally rejected what she ate: she lost flesh, though her desire for food and drink was excessive: and being indulged in all her most injurious fancies, she often vomited ten or fifteen times in a day. She lay, for the most part, doubled up in bed; or sat on a chair, with her legs drawn up and her abdomen pressing on her knees. Her bowels were obstinately costive, and her sufferings became extreme. She did not pass above three ounces of urine in the day. Complained of pain in the back, passing through to the crest of the ilium, on the right side. During the last few hours of life, she had three fits of a convulsive character; but remained sensible till her death, which occurred February 13th, 1836.

The interest of the foregoing case consists partly in its singularity; partly in the exact accordance of the symptoms during life, with the appearances after death; and partly in the warning which it affords us, that, in cases where experience has shewn that a patient cannot exert himself, or call any function into action beyond a certain point, without suffering, we should have recourse to restrictive as well as remedial measures in our treatment; for it often happens, in cases which afford much fewer indications of structural peculiarity than were evinced in the present instance, that such constitutional failure exists as to defy the beneficial action of remedies. There is little doubt, that had this young woman been able or willing to avoid over exertion or irregularity in diet, and contented herself with such an extent of exercise as the peculiar nature of the organs of respiration and digestion would allow, she might have remained in the comfortable enjoyment of life; and not only avoided the perpetual recurrence of severe attacks of dyspnœa and vomiting, but have prevented that disorganization of the stomach which, ultimately, had a large share in the fatal result.—*Guy's Hospital Reports.*

It is strange that Dr. Bright has not remarked on the important bearings which this case has on the question, whether the stomach, in the act of vomiting, ejects its contents in virtue of its own power, or whether the ejection is produced by compression exercised on it by the abdominal parietes, as argued by Majendie and others.—Editors of Dublin Journal.

On the Employment of Muriate of Barytes in the Treatment of White Swellings, by M. Lisfranc.—The "Gazette Médicale" reports a clinical lecture of M. Lisfranc's, in which he relates the results of his experiments with this medicine, which has been long known, but has been recently brought into notice by M. Pirondi, of Marseilles.

Six grains of muriate of barytes are dissolved in four ounces of

distilled water, of which one spoonful is taken every hour, except one hour before and two hours after each meal. In order to tolerate the medicine, the patient must abstain from wine and meat, taking only water and vegetable food. The bottle should not be exposed to the sun, or the salt will be precipitated, and the last spoonfuls contain a greater quantity; to avoid this, it should always be shaken. Sometimes the medicine produces slight pain in the stomach, or a feeling of weight; but if other symptoms do not follow, the stomach gradually becomes accustomed to the remedy, and the pain ceases. If, on the other hand, nausea, vomiting, or even some slight symptoms of poisoning come on, the medicine should be suspended, and cautiously resumed. The climate has some influence; for, although at Marseilles two drachms have been given, M. Lisfranc has never been able to increase the dose in Paris beyond forty-eight grains, and often he has been unable to reach that. The unpleasant symptoms have been removed by whites of eggs. Numerous patients have been submitted to this treatment, and the following are the conclusions which M. Lisfranc has arrived at.

1. Generally the white swelling has been much amended, and sometimes cured. 2. The benefit has been greatest amongst the scrofulous. 3. In some very few cases the muriate alone has cured. 4. After a certain time, the disease having become stationary, it was necessary to employ another method. At a later period, the renewed use of the muriate has produced excellent effects. 5. It may be employed both in the acute and chronic stage of white swellings. 6. Serious accidents have never resulted from its use; the slight symptoms before mentioned have always yielded readily. 7. A frequent effect is a diminution in the frequency of the pulse; this falling from sixty or eighty to forty or fifty, or even to twenty-five. 8. In some circumstances the medicine, continued at the dose of twelve grains during the month, has produced as much amendment as in other cases where the dose has been gradually augmented. 9. Where the patients have been slightly inconvenienced with the medicine, it has been most useful. 10. Compression and local abstractions of blood have been often combined with this treatment, and with extreme advantage.

M. Lisfranc considers muriate of barytes, given according to M. Pirondi's method, as a truly valuable acquisition to surgery, ("une varie conquête chirurgicale.")—*Gazette Médicale de Paris*, No. 14. 2 Avril, 1836.

On the Cure of Intestinal Fistulæ by the Actual Cautery, by Dr. Fingerhuth.—The success attending the employment of the hot iron in the cure of artificial anus, already recommended by Dieffenbach, is confirmed by two cases related by Dr. Fingerhuth.

In both, abdominal inflammation, caused by violent blows, had been followed by external abscess, to which succeeded discharge of fæcal matters. Various cauteries were employed to destroy the membranes lining the fistulæ, and to convert them into granulating surfaces, but without producing their complete obliteration. The

fistulous openings, although somewhat diminished by imperfect granulations, showed no tendency to become closed. Cauterization was then adopted by means of an iron, corresponding in diameter to that of the fistulæ, and the temperature of which was scarcely elevated to that of red heat. Luxuriant granulations soon covered the cauterized parts, the fistulæ diminished, and the surfaces being again destroyed by a heated iron corresponding in size to the apertures which remained, they were eventually cured.—*Wochenschrift für die gesammte Heilkunde*, No. 6, 1836.

History of a successful Case of Cæsarean Section, by Dr. Meyer, of Minden.—This is the fourth time Dr. Meyer has performed this operation, and three of his cases have terminated favourably. The subject of the following was the wife of a shoemaker, named Holle, residing at Minden, a woman of small size and slender make, aged thirty-eight, who had previously enjoyod good health, and borne three children easily and without any bad consequences. After this she was attacked with an arthritic affection, which, from poverty, privations, grief, and want of timely assistance, had increased to such a degree, that, for the space of a year, she only left her bed occasionally, and was then merely able to crawl about her room, bent double and holding by the chairs.

In this state she became again pregnant ; an occurrence, of which the first intimation was given by the motions of the fœtus in utero. On the evening of the 19th of June, labour came on, and towards morning, the midwife in attendance, having discovered an abnormal state of the pelvis, advised her to have the assistance of an accoucheur. A neighbouring physician, Dr. Heilbronn, was called in, who, finding the pelvis so deformed as to render artificial delivery by dismemberment, or the Cæsarean section unavoidable, ordered the patient to be blooded, and requested Dr. Meyer to take charge of the case.

An examination *per vaginam* showed that the pelvis was excessively deformed. On introducing the finger, which was done with difficulty, owing to a bending inwards of the ossa ilii and pubis, the summit of a round solid body was felt, which at first might be taken for the head of the child, but a more accurate examination proved it to be the distorted promontory of the sacrum, which projected close to the retreating symphisis pubis. Hence it was immediately concluded that the child lay entirely in the false pelvis ; a circumstance which also explained the extreme prominence of the belly, which rested with the navel touching the external parts of generation.

Besides that the child was still living, the impossibility of getting at it through the vagina determined Dr. Meyer to abandon at once the idea of dismemberment, and he decided on this plan of operation, which the mother also pressed for ; although, from her unfavourable condition, there was little hope of saving her. The further proceedings are thus described by Dr. Meyer :

" The patient was carried to the table, and placed on a straw mattress. The thighs could be separated only so far as merely to afford room for the hand of an examiner ; the lumbar vertebræ appeared to

be anchylosed with each other and with the sacrum, and it was impossible to place her in an horizontal posture. In this half-sitting posture, the abdomen presented a space of only about four inches from the umbilicus to the pubes. Notwithstanding this, and the presumption that the placenta lay in the line of the proposed incision, I made choice of it for reasons which seemed to me conclusive.

"Two assistants stood, one on each side of the patient, with large soft sponges, warmed and slightly oiled; while Dr. Heilbronn stood at her feet, ready to hand the instruments and take hold of the child. The abdomen was then firmly and powerfully drawn up, and an incision, about four inches in length, was made through the integuments, commencing as close as possible to the umbilicus, and terminating at the pubes. The division of the linea alba and the peritoneum, to the same extent and in the same direction, gave exit to a considerable quantity of water, showing the coexistence of ascites. The discharge of this fluid, however, diminished the enormous tension of the abdomen, and contributed to facilitate the delivery. In all my former cases I had found the uterus extremely thin, scarcely thicker than stout paper, and easily divided: in the present instance, however, the walls were unusually firm, and three lines in thickness. A new obstacle here arose: in cutting through the uterus, the knife entered the placenta, which was attached exactly opposite the line of incision. The consequences were, a division of the larger vessels which are collected about the centre of the placenta, giving rise to a considerable hæmorrhage, and a further interference with the previously limited space for the introduction of the hand and the passage of the child. No time, however, was to be lost: the section of the anterior wall of the uterus was speedily completed, the necessary detachment of the placenta was accomplished without any excessive hæmorrhage, and the right knee of the child presented. As the opening in the womb was scarcely large enough for the passage of the head, it was necessary to extract the arms; after which the head followed, not without some resistance, yet without any laceration of the uterus. The extraction of the child was followed instantly by a remarkable contraction of the womb, and the bleeding stopped.

"Vomiting came on, accompanied by spasmodic contractions of the abdominal muscles, so that the assistants were obliged to direct their whole attention to the intestines, which were protruded with great force, and which were returned and kept in with much difficulty by the oiled sponges. When the blood and aqueous fluid were removed from the cavity of the abdomen, the uterus was seen contracted to about the size of two fists; and, as the somewhat swollen edges of the incision were tolerably approximated, the external wound was united by the bloody suture without further delay. After it was finished, a repetition of the vomiting forced a piece of omentum, about two inches long, through an interval between the sutures, two inches below the umbilicus: this was removed with the scissors without any hesitation. Immediately over the pubes an opening was left, about an inch in length, into which a tent of lint was inserted; compresses were placed at each side of the wound, a twelve-tailed bandage applied, and, in half an hour from the commencement of the operation, the

patient, who during the whole time neither stirred nor uttered a single moan, was laid comfortably in bed, the vomiting had ceased, and the cough was less troublesome. The child, a male, come to its full time, and twenty inches in length, but imperfectly nourished, had breathed immediately after delivery, cried, and was doing well. About half an hour afterwards, during a violent paroxysm of coughing, a full half-ell of intestine was forced through the opening at the lower end of the wound, and lay between the thighs distended with flatus. Dr. Meyer, unwilling to undo the bandage in the absence of proper assistants, fortunately succeeded in reducing it by the taxis, and secured the opening with a compress and strips of adhesive plaster."

The patient ultimately got quite well.

From the successful termination of this and two other cases, Dr. Meyer is led to the conclusion that the Cæsarean section is much less dangerous than is generally supposed; that, when we have recourse to it in time, and under favourable circumstances, it is oftener followed by a happy result than other violent modes of artificial delivery, adopted in case of great contraction, and abnormal formation of the pelvis; and that, generally speaking, when there is a great probability of saving the mother, it should be preferred to dismembering the living child. Leaving the question undecided, whether the lateral incision should be selected, because the section of the anterior wall of the uterus is likely to meet the placenta, his experience inclines him in all cases to make choice of the linea alba. Finally, he corrects a statement made by him in a paper inserted in Siebold's Journal, 3d Band. 1st Heft, that "in such operations, only one intelligent assistant is necessary;" and declares it to be his conviction, that three intelligent assistants are requisite,—two to prevent the protrusion of the intestines, and a third to remove the placenta and fœtus.—*Neue Zeitschrift fur Geburtskunde.* Band. iii. Heft. 1. 1835.

Severe Symptoms from the Bite of a Spider.—A young girl gleaning in the fields, was bit above the left bosom by a large dark-coloured spider. She felt a sharp pain in the part at the time; and in the course of a few minutes she became so weak that her limbs sunk under her, and her sufferings were so great that she rolled about on the ground, and could not refrain from screaming out. When Dr. Hameau visited her in about an hour after the accident, he found her drenched with perspiration; her face alternately pale and flushed, her extremities cold, the breathing slow and oppressed, and the pulse irregular and very small. She complained of severe pains in the feet, knees, thighs, and in the small of the back; and as these subsided, they fixed themselves in the epigastric region, causing a sense of most distressing oppression and anxiety. The muscles in several parts of the body were in a state of continual oscillation, or tremor: firm compression of the limbs afforded considerable relief to this symptom. The seat of the bite was red and swollen, and a small vesicle filled with a yellowish serum occupied its centre.

Dr. H. opened this vesicle, and moistened the part with some

fluid ammonia, and ordered the patient to have a warm bath and a powerful opiate. The symptoms however were not relieved, until three doses of opium had been exhibited. The pains then abated, and a general reaction came on. Towards evening, however, the symptoms were partially renewed, and three grains more of opium were given. No narcotism was induced. On the following day, the patient was altogether much better. Some degree of salivation, and also a papular eruption, made their appearance; but these symptoms quickly ceased, and the girl was soon quite well.

Poisoning from a Viper-bite—Cure with Sulphate of Quinine.—A countryman, forty-four years of age, was bit by a viper between the fore-finger and thumb of the right hand. On the following day, when admitted into the hospital, the whole extent of the right arm was enormously swollen, and its surface was of a livid red colour. The face and trunk had a jaundiced hue, and the extremities exhibited diffused patches of redness. There was great prostration of all the vital powers; the pulse was scarcely perceptible; the body was bedewed with a cold clammy sweat; the face was convulsed, the pupils dilated, the breathing laborious, and there were frequent efforts of vomiting. Dr. Butazzi calling to mind the results of some cases recently published in the Filiatre and in the Revue Med., prescribed quinine in large doses, three grains to be taken in a spoonful of wine every hour.

On the following day, the arm remained as much swollen as before, the pulse was scarcely to be felt, and the patient complained of pain in every part of his frame: the dose of the quinine was increased to four grains. On the next day there was a decided amendment; a copious warm perspiration had taken place, the discoloration of the surface abated, and the features were recovering the character of health. The swelling of the affected arm gradually from this time subsided, and, under the use of oily embrocations, altogether disappeared. Two drachms in all of the quinine were taken in the course of between two and three days.—*Filiatre di Napoli.*

Spontaneous Combustion.—An instance of spontaneous combustion is reported in the French papers to have taken place at Aunay, in the department of Avalon. A very fat woman, aged seventy-four years, and addicted to drinking brandy at 27 degrees, lived alone, and one evening returned home as usual, but, as she did not appear among her neighbours the next morning, they knocked at her door. No answer being returned to repeated demands, they summoned the mayor, who forced the door and exposed a horrible spectacle, accompanied by an extraordinary smell. Near the chimney lay a heap of something burned to cinders, at one end of which was a head, a neck, the upper part of a body, and one arm. At the other end were some of the lower parts, and one leg, still retaining a very clean shoe and stocking. No other traces of fire were to be seen, except a blue flame which played along the surface of a long train of grease, or serous liquor, which had been produced by the combustion of the body. The mayor found it impossible to extinguish this flame, and sum-

moned all the authorities; and, from the state of the apartment and comparison of circumstances, it was concluded among them that, previous to going to bed, for which she had evidently been making preparations, the woman had been trying to ignite some embers with her breath. The fire communicating with the body by means of the breath, combustion probably took place, and would appear to confirm an opinion entertained by several learned men, that that which is called spontaneous combustion of the human frame, never takes place without the presence of some ignited body near the person predisposed to combustion. A surgeon who bled an habitually drunken person, accidently put the blood extracted near a candle, when immediately a blue flame appeared on the surface, which he found extremely difficult to extinguish.—*Athenæum*, July 30th.

Injections of Gaseous Chlorine in Hydrocele—Treatment of Chilblains.—We are informed that Dr. Deblois of Tournay, has treated successfully a great many cases of hydrocele, by using chlorine as an injection into the vaginal sac in lieu of wine, or other stimulating fluids. We are not aware that any trial of this remedy has been made in this country, and as we are told that it has answered admirably well in the practice of Dr. D. it may deserve to be introduced to the notice of the English surgeons. The gas is to be permitted to remain for two or three minutes in the sac; and if sufficient irritation is not induced by the first dose, a second or third insufflation is to be practised. One advantage which it is said to have over the injections in ordinary use is, that the gas penetrates into the sac more uniformly and irritates equally every part with which it comes in contact. The narrator recommends the use of these gaseous injections in that form of goitre which has of late years been called hydrocele of the neck.

In concluding his remarks on chlorine, he praises in high terms a lotion composed of one part of liquid chloride of lime and five of water as a very effectual remedy against chilblains, whether they are ulcerated or not. The affected part is to be kept constantly wetted with the lotion.—*Bulletin. Med. Belg.*

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* Erratum, p. 93, for Dr. J. Thompson, read Dr. J. Morrison, &c.

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